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COURSE OF LECTURES
ON
DISEASES OF THE SKIN;
THEIR
HISTORY, PATHOLOGY, AND TREATMENT.

Delivered in the Middlesex Hospital, during the Summer Session, 1842, by

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LECTURE XIII.

THE diagnosis of small-pox is a question of the highest importance, but one not easy of solution. The precursory fever of small-pox, in cases of moderate severity, differs in no striking particulars from simple fever. It is true that the nervous depression is greater in all affections depending on the reception of a morbid poison than on such as result from simpler causes; but the distinction between febrile symptoms alone is not sufficiently obvious to enable the practitioner to predict, in the early stage of the variolous disease, the nature of the affection, without collateral circumstances to guide his judgment. The most prominent of these collateral circumstances is the prevalence of small-pox, or the previous exposure of the individual to fomites of infection. The pains in the loins are regarded by Chomel as pathognomonic; the pains in the limbs, moreover, are somewhat greater in small-pox than in other eruptive fevers; and there is a greater tendency to convulsions among children. On its first appearance the dotted character of the eruption resembles rubeola, but the elevated and granular nature of the points affords a strong ground of diagnosis. When once developed, small-pox is so characteristic as to render the possibility of confounding it with any other cutaneous disorder very unlikely.

CAUSE.—The cause of small-pox is a specific infection or contagion, but the origin of the variolous poison is unknown. Like the allied eruptive fevers, measles and scarlet fever, our knowledge of the early history of the disease is derived from the writings of the Saracens, of Ahrun, Isaac, Judæus, Georgius, and especially of Rhazes. During the siege of Mecca in 569, the small-pox committed serious devastation in the army of the besiegers; subsequently, we find it raging in Egypt, and still more recently pursuing the track of the Saracens through Italy, Spain, and France. From the Saracens the disease was communicated to the Crusaders, and by these latter was spread throughout Europe. But, although the European origin of the disorder is so comparatively recent, there is every ground for the belief that the small-pox was known in the East at a period far antecedent to that which is recorded as its Asiatic origin.

It has been maintained by some authors that the cutaneous eruption of small-pox is the effect of the generation of myriads of minute animalcules in the skin. There is, however, no ground whatever for such a belief.

Small-pox occurs at all periods of life, from that of the fœtus in utero to the last term of existence; it affects the sexes equally, and appears at all seasons and in all climates. Epidemics of small-pox, however, are more frequent in the summer and autumn than in other seasons of the year. It may invade either in the sporadic or epidemic form, and as a general rule, affects the same individual but once in his life-time. To this general law there are, nevertheless, numerous exceptions; instances are by no means rare in which secondary small-pox exists, and cases are on record in which the disease has attacked the same person three, four, and even a sixth time. Generally the secondary attack is much milder than the first; but in rare instances it has been equally severe with the preceding.

The *prognosis* of small-pox varies with the nature of the eruption. When the latter is discreet and the constitutional symptoms moderate, the prognosis is favourable; but when the opposite conditions are present, the worst results are to be apprehended. A mild attack of small-pox terminates in from two to three weeks; but the disease in its severer

forms, being prolonged by dangerous sequelæ, may endure for months. Circumstances unfavourable to the progress of variola are affections of the brain or respiratory organs; signs of cachexia, as evinced by sanguinolent pustules, variolæ nigræ, as they have been called; petechiæ; mental depression, resulting from alarm, or anxiety; plethoric habit of body; sound health; or irregularity of development of the pock.

TREATMENT OF SMALL-POX.

The treatment of variola, as of other eruptive diseases, calls for the exercise of the highest amount of judgment and discretion. In the milder forms of the disease, as of the most severe, your remedies must be directed to regulate or oppose the symptoms as they arise; your treatment must be rationally expectant. Where no unusual indications are present, the patient must be confined to his bed, kept in a state of repose as complete as possible; the temperature of the apartment must be equable and cool; the body-linen must be changed as frequently as convenience admits, and the patient supplied abundantly with diluents. Should congestions arise, our means of relief are local abstraction of blood and counter-irritation. Purgatives must be eschewed, and laxatives when needed, used warily, and carefully selected. The treatment of small-pox, at the decline of the disorder, calls for the use of wine and tonics, and these should be administered until convalescence is fairly established.

Small-pox is no subject for showy treatment; you must be wary and think, and, like steady pilots, be careful to avoid the rock and shoal rather than aim at brilliancy and effect. During the primary fever, almost nothing save hygienic management is required, but this needs all your care, for you will find a thousand prejudices to contend with in the public mind. A valuable medicine at this period and during the whole continuance of febrile symptoms, is the liquor ammoniæ acetatis, and beyond this you can hardly advance with safety. During the secondary fever, if the bowels be confined, recourse may be had to cooling salines, either in the state of effervescence or already combined. The bitartrate of potash or the tartarised soda are well adapted to this purpose.

Should symptoms of inflammatory disturbance of the cerebro-spinal system be apparent, you may apply leeches to the mucous membrane of the nose or behind the ears, with mustard-baths or plasters to the legs and feet. If the mucous membrane of the fauces be swollen and inflamed, acid and astringent gargles may be used with advantage. If the eyelids be painful and tumefied, you must employ warm fomentations and emollients. You will sometimes find the eyelids so much swollen and so tense as to defy your powers of separating them until they have been fomented; and then, perchance, as soon as they are slightly parted, a burst of purulent secretion will escape from beneath them, showing the necessity of keeping them in such a state as will enable this fluid to flow away as it is produced. I have found olive oil in these cases promote the object I had in view, and I advise you to try it under similar circumstances. When there are pains in the chest or at the epigastrium you must apply leeches over the painful part, and if the epigastric pains are accompanied by vomiting and denote gastritis, abstraction of blood is the more urgent.

Should the eruption be tardy in its appearance, it may be well to endeavour to call it forth by the mustard-bath, and by the employment of diaphoretics. Narcotics are objectionable during the primary fever of small-pox, but are valuable means of relief to the restlessness and want of sleep which often accompany the secondary fever. Sydenham was an advocate for a small bleeding at the commencement of the secondary fever, and this he followed up with an opiate. Bleeding, however, has fallen into disrepute, and although it may be practised at any period of the disease without danger, it is a procedure, at the present day, rarely employed. Of course I make exception of local bleedings by leeches, for the relief of visceral congestions.

Counter-irritation is an important remedy in the visceral congestions which occasionally complicate small-pox. Of our means to this effect the mustard-plaster is certainly the best application; stimulating liniments follow next; but blisters, both in small-pox and the allied eruptive fevers, rubeola and scarlatina, are objectionable on account of the diminished vitality of the skin, and the tendency to sloughing which exists. Another mode of applying counter-irritation which may be sometimes beneficially resorted to, is the administration of mild purgatives; but the effects of purgatives require to be watched, because of the irritation of the mucous membrane of the alimentary canal which already prevails.

Excessive pain and heat of head, with delirium, are sometimes present as a complication of the usual course of small-pox. When these symptoms depend on congestion they may be removed by the ordinary means applicable to that state, namely, abstraction of blood from behind the ears, and from the mucous membrane of the nose by leeches, and mustard-plasters to the nape of the neck. But when they resist this treatment the application of cold will probably be successful. The best means of applying cold is by pillows, filled with cold water, and frequently changed, or the cold cushions described by Dr. James Arnott in the second volume of THE LANCET for 1841-42, p. 441.

Where the symptoms denote cachexia a

tonic treatment must be pursued, and the use of wine adopted. In such a state there is much difficulty in maintaining the powers of the system. The same remarks apply to the extreme nervous depression which sometimes accompanies variola, as, for example, in that variety termed by Dr. Gregory nervous variola.

Cold affusion has been advocated in small-pox, as in the other eruptive fevers, but the practice has not been generally pursued, on account of the danger of determining congestion of the mucous membrane. The danger, however, is not so much in the remedy as in the mode of its application; the time for its use must be properly noted, the state of the nervous powers of the patient must be known, and then it must be used boldly, and without improper exposure of the patient. When all the indications are present which are favourable to its use, cold is a valuable sedative, but improperly applied it may occasion further depression of the nervous powers and a fatal issue. Sponging with cold or tepid water is a compromise with affusion, it is highly grateful to the patient, and beneficial to the disorder, provided always that the patient be not chilled by the manner of its application. Cold does not seem to have the effect of repelling the eruption, nor, indeed, should we be led to expect such an effect, reasoning physiologically, unless, be it always remembered, the cold is carried to the extent of producing nervous depression, and then you will have the consequences of nervous depression, one of which is arrest of development of the eruption, with retrocession of the congestion. The popular prejudice against the influence of cold is, therefore, a prejudice limited by philosophical laws; but as it is ever more easy for the human mind to retain a prejudice than to work the problem on which the fragment of truth of the prejudice rests, the prejudice must be respected, lest by destroying it we destroy, at the same time, the truth which gives it existence.

It has been proposed, as a method of treating small-pox, that the patient should be vaccinated as early as possible in the disease, with a view to the modifying influence of vaccinia. For it has been observed that the progress of the latter essentially mitigates the violence of the former, even when practised after the commencement of small-pox. This procedure was first proposed by Eichhorn, who was in the habit of introducing the vaccine lymph by a great number of incisions as soon as the presence of small-pox was apparent.

I now come to the local treatment of small-pox, and one of the first instructions I would urge upon you is to prevent the tearing of the pustules by the hands of the patient in attempts to relieve the itching which attends the periods of maturation and desiccation. This care is particularly needful in the case of children in whom mental control is unformed, and also in adults during sleep, where the partial wakefulness of the mind urges to the commission of acts which the sleeping judgment is incapable of controlling. It is customary, under these circumstances, to muffle and confine the hands, but while you are so doing you must employ every means in your power to calm the local irritation. With this view, the skin, particularly that of the face, should be bathed with a fomentation of poppy-heads, or any demulcent fluid, such as infusion of mallows or barley water; any secretions which are poured out should be removed by a sponge moistened with these fluids; and if the surface be abraded it should be dusted with some absorbent powder. Comfort is also frequently attained by anointing the surface with oil or cold cream.

The Arabian physicians were wont to open the pustules and remove their contents, and they found that this practice was attended with the most beneficial results. The process is tedious, but when it is of importance that scars should be prevented, there is no plan so well calculated to effect the object. When the pustules are opened the pus may be absorbed by means of a sponge wetted with warm water, or with a decoction of poppy-heads.

The most important measures in the local treatment of small-pox are those which have been termed *ectrotic*, a term derived from the Greek word *εκτιτρωσκειν*, to render abortive. The first of the ectrotic plans to which I shall allude, is that in which nitrate of silver is the ectrotic agent. It is to MM. Serres, Bretonneau, and Velpeau, that we are chiefly indebted for a knowledge of the effects of this salt; and it would appear from their experiments that if the vesicle of small-pox be opened at an early period, say on the second day of eruption, and a point of nitrate of silver inserted into the aperture, the further progress of the pock will be instantly arrested. Two objections to this remedy, however, soon made themselves apparent; firstly, the great length of time required to ectrotise a considerable number of pocks; and, secondly, the severe pain resulting from the multiplication of the operations. To obviate these objections it was proposed to apply the nitrate in the form of solution over the whole surface to be ectrotised. But it was found that, employed in the latter manner, the salt was utterly useless, that it masked the progress of the eruption from sight without impeding its development. The nitrate of silver, therefore, can only be considered as a remedy of partial application, and fitting to be used upon such parts of the body of small extent as would be most seriously deformed by the existence of scars. The pain which it creates would be an obstacle to its more extensive use; the length of time required would occasion another

objection; and a still more serious one would be the irritation which it excites in the skin. It was, moreover, ascertained that unless used quite at the outbreak of the disease, its application was ineffectual in stopping the progress of the pock.

Another ectrotic remedy has been recently suggested by Dr. Midivaine, of Ghent, namely, sulphur ointment rubbed with moderate friction into the skin. The strength of the ointment, according to Dr. Midivaine, should be two drachms of sulphur to the ounce of lard, and should be applied at as early a period as possible of the eruption. When used in this manner Dr. Midivaine declares that it produces a hardening and corrugation of the pocks, the tension and swelling of the skin subside, the gastro-intestinal irritation abates, and the patients are quickly restored to appetite and strength.

Another project for procuring the arrest of development of the pocks of variola is the exclusion of light. Dr. Picton, of New Orleans, asserts that no instance of pitting accrued in those patients from whom the influence of light was excluded; and M. Serres alludes to his successful practice in small-pox, during a period when his patients were confined to a dark and ill-ventilated apartment, more like a cellar than the ward of a hospital, in La Pitié. The same principle seems to have suggested to M. Legrand the idea of coating the whole surface of the skin with gold-leaf, a proposal which that gentleman made to the Academie de Medicine in 1839. I should scarcely have thought it necessary to direct your attention to this subject, however, were it not for some experiments relating thereto, performed by M. Serres. With a view to ascertain the influence of light and air on the eruption of small-pox, this gentleman placed a glass capsule over a pock, and found that the pock was modified in its growth; that where light and air were partially interrupted the modification was partial, but that when they were wholly excluded, the pock became speedily shrivelled and desiccated.

The most valuable of the ectrotic means of treatment of small-pox I have now to mention; it is one which served as the subject of an essay read before the English Medical Society of Paris by their president, Dr. Olliffe, and was published in the first volume of The Lancet for 1840-41. This remedy is mercury applied externally. The modifying influence of mercury over small-pox when taken internally has long been admitted, but it remained for modern times to show that a beneficial effect could be produced by its external application. This has been put to the test of experiment by M. Serres, and the experiments of this gentleman have been followed up by M. Briquet. The formula employed in these experiments is a plaster, called *emplastrum vigo cum mercurio*, of which the constituents, according to the French Pharmacopœia, are the following:—

℞ *Mercury*, 95 parts;
Common plaster, 312 parts;
Balsam of storax, 48 parts;
Wax, *resin*, *turpentine*, of each 16 parts;
Gum ammoniac, *bdellium*, *olibanum*, and *myrrh*, of each, 5 parts;
Saffron, 3 parts;
Spirit of lavender, 2 parts. M.

In repeated experiments made with this plaster it has been found that, if applied upon the skin at an early period of the eruption, the eruption shrivels and dries up, and the inflammation of the skin subsides. Even at an advanced period the eruption seems to undergo a check, and certainly ceases to make further progress. To ascertain how far these effects may have resulted from merely covering the skin so as to exclude the light, a portion of diachylon-plaster was placed on one arm, while the vigo-plaster was applied to the other. Under the former no modification whatever was produced; but under the latter the usual ectrotic changes took place. In another case the face was covered with this plaster, but the patient during the night tore off a portion. Well-formed pustules were developed on the exposed surface of skin, but none were found beneath the plaster when it was removed.

Dr. Olliffe recommends that the whole face should be covered with this plaster, leaving spaces for the eyes, nostrils, and mouth, and that such portions of the skin as may chance to remain uncovered should be anointed with mercurial ointment. In the absence of the plaster, mercurial ointment might be substituted. It is of the utmost importance that the plaster should be employed early in the eruptive stage, say on the second day, as after this period the chances of a successful issue of the experiment are much invalidated. The period for retaining the plaster, according to Dr. Olliffe, is three days in discreet, and four in confluent variola. This I think hardly sufficient. According to M. Briquet "the mercury acts as an antiphlogistic or resolutive in destroying or suppressing the local inflammatory process; or it exercises a specific action on the cause, whatever it be, which produces the variolous pustule."

REMOVAL OF A TUMOUR BY VACCINATION.

M. Pigeaux presented to the French Acad. de Medicine a child in whom an erectile tumour on the forehead had spontaneously dispersed after vaccination had been practised on it topically. This is not the first case that has been adduced of such an effect resulting from the action of the vaccine lymph.

LIEBIG,

HIS CHEMISTRY AND REVIEWERS.

BY

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No. IX.

The facts and opinions contained in Liebig's work relating to the function of respiration and the production of animal heat,

are those which I purpose, in the present paper, to lay before my readers. The subject has excited great interest, and it appears that some misapprehension must exist as to the "credit of originality" really due to our author under this important head.

The position, that the effect of respiration is to generate heat by a process analogous to combustion, advanced by Mayow, confirmed by the discoveries of Black, Lavoisier, and Laplace, and modified in its explication by those of Crawford, was accepted by physiologists, notwithstanding the existence of certain apparent anomalies, as the true account of the efficient cause of animal heat, until the more recent experiments of Despretz, Dulong, and others, cast a doubt upon the doctrine, and induced many physiologists to refer the phenomenon wholly, or in part, to the influence of the nervous system.

According to the original theory, the combustion of carbon and the production of carbonic acid gas, or the combustion of carbon and hydrogen, which carbon and hydrogen are derived from the food, is the sole cause of animal heat. According to the opinions more recently entertained, the whole of the heat developed in the living animal is not the consequence of the combination of the oxygen of the air with carbon and hydrogen, but a part of it is due to "nervous influence," or some other cause. The latest account of this complex theory is given by Dr. Collier, in the following words*:—"There are three modes by which heat is probably generated within the body. First, by the chemical decomposition which takes place in respiration; secondly, by the influence of the brain and nervous system, in some degree, perhaps, analogous to its development by galvanic influence; and, thirdly, by the process of digestion and nutrition." The latter explanation of the phenomenon, or something equally complicated, and closely allied to it, is advocated by those who deny the heat developed in an animal body to be wholly attributable to a process analogous to combustion.

Liebig maintains, simply, and in spite of the experiments of Despretz and Dulong, and to the contrary of all the counter observations, which he believes to be fallacious, that the mutual action between the elements of the food and the oxygen of the air conveyed by the blood to every part of the body, is the sole source of animal heat.

This conclusion is supported by experiments and observations derived from numerous sources, and quoted from various physiologists. It is supported, also, by original facts and observations, and by the method of research, peculiar to the author, which has led to such remarkable results in other departments of animal chemistry. Without further preface, then, I proceed to the subject as propounded in the work itself.

Experience teaches that the animal body is a heated mass, which bears the same relation to surrounding objects as any other heated mass, receiving heat when they are hotter, and parting with it when they are colder, than itself. All living beings who respire oxygen possess within themselves the source of this heat, and the manifestations of the vital force are dependent on the evolution of a certain quantity of it; that is to say, upon a certain elevation of temperature. (Paper I. p. 233.)

It is only in those parts of the body to which arterial blood, and with it the oxygen absorbed in respiration, are conveyed, that heat is produced. Hair, wool, or feathers, do not possess an elevated temperature. By far the greatest portion of the carbon received daily into the system, as a constituent part of the food, and some of the hydrogen (whatever intermediate changes they may undergo), are ultimately given off in the form of carbonic acid gas and water. The great bulk of the oxygen, carbon, and hydrogen, are eliminated from the system in this manner only.

It is known that in whatever way carbon may combine with oxygen, the act of combination cannot take place without the disengagement of heat. Whether the combination takes place rapidly or slowly, at a high or at a low temperature, the amount of heat liberated is a constant quantity. The carbon of the food, accordingly, which does not pass off by urine or fæces, and which is converted into carbonic acid gas within the body, must give out exactly as much heat as if it had been directly burnt in the air, or in oxygen gas; the only difference being that in oxygen the combustion takes place more rapidly, the heat becoming more intense in a given time.

We are acquainted with a large number of processes of eremacausis, in which the elements of carbonic acid gas and water are separated from certain pre-existing compounds. These processes are uniformly accompanied

* LANCET, 1841-42, vol. ii. p. 724.

by a disengagement of heat, exactly as if the carbon and hydrogen had combined directly with oxygen. Examples are given in Paper III. The union of oxygen with carbon and hydrogen in the living system is precisely analogous. It is an eremacausis, or more or less rapid combustion of these combustible materials. Whatever different forms the materials assume in the progress of their combustion in the animal economy, or elsewhere, they cannot produce more heat than if directly burned in a furnace, nor do they produce less.

It is clear, then, that the carbon and hydrogen of the food, which are converted within the body into carbonic acid gas and water, must give out exactly as much heat as if they had been directly burnt. At the temperature of the body the affinity of hydrogen for oxygen is greater than that of carbon, and the deficiency of hydrogen is the only reason why carbonic acid gas is the chief product.

Thus, as originally held, the disengagement of heat in an animal body is manifestly the result of the combination of a combustible substance with oxygen. But Liebig affirms that if the consumption of food be the first condition of animal life, the absorption of oxygen is the second, and that the production of animal heat is *uniformly, and under all circumstances, an act of eremacausis, or combustion.*

The most important exception taken to this conclusion is the fact, determined by experiment, that more heat is generated by an animal in a given time than can be accounted for by the oxygen inspired.

According to Despretz, every three and three-quarter cubic inches of oxygen gas, when converted into carbonic acid gas, give out 1° of heat; and every three and three-quarter inches of oxygen gas, when combined with hydrogen, evolve 1° also of heat; accordingly, the heat evolved during the combustion of carbon and hydrogen is proportional to the quantity of oxygen gas consumed.

When an animal is made to breathe in an apparatus so contrived that the quantity of oxygen gas inspired, and that of the heat evolved, can be accurately measured, the latter exceeds the former by a considerable amount. In the first place, more oxygen is inspired than carbonic acid exhaled, and this excess of oxygen must be referred to the combustion of hydrogen. In Despretz's experiments the heat evolved exceeded by at least one-tenth, and, upon an average, amounted to one-fifth, of that which could be accounted for by the combustion, both of the carbon and hydrogen, as measured by the quantity of oxygen absorbed. The result of Dulong's experiments is, that the oxygen inspired will account for eight-tenths only of the heat given out.

Liebig gives this reply to the experiments of Despretz. The animals were kept in a constrained position, at a temperature of 98° or 99°, surrounded by water below 50°; the result, therefore, only proves, that when motion is restrained, and a great difference exists between the temperature of the body and that of the surrounding medium, more heat is given off than corresponds to the oxygen consumed. Liebig remarks again, that had the windpipes of the animals been tied, the strange result would have been obtained of a rise in the temperature of the water without any consumption of oxygen.

Another reply to the objection at once suggests itself. Referring to Dulong's experiments, which were very similar to those of Despretz, and led to the same conclusion; the utmost duration of either of them was two hours and thirteen minutes. Some were continued but little beyond an hour. The animal was placed for a period within this range, under uniform and very confined circumstances, so that it cannot for one moment be assumed that the quantity of oxygen inspired was a measure of what might have been inspired for hours preceding the experiment. It by no means follows that the whole of the oxygen inspired by an animal in a given very limited period, as, for instance, two hours, is precisely that oxygen which produces the whole of the heat extracted during the same period. We have to remember that the processes in the living body are not processes of open active combustion, but of slow combustion, or eremacausis; and it is by no means clear, had the animals experimented upon been kept under the artificial circumstances of the experiments for a period of long duration, that they would have continued to evolve the excess of heat. On the contrary, the fact cited by Liebig (p. 38), that when animals are made to respire in an unnatural position, so as to preclude motion, their temperature sinks in a degree appreciable by the thermometer, is conclusive that such would not have been the case.

The explanation of the fact that when the windpipe is tied heat continues to be generated (?), most assuredly is, that the oxygen previously absorbed by the blood, and contained in the system, proceeds in its work of eremacausis for some time after its inspiration.

In death, under ordinary circumstances, the body continues to generate heat for a long time after the cessation of respiration. On proceeding to post-mortem examinations from twenty-four to thirty-six hours after death, I frequently find the body perceptibly warm. The same observation has been made three days after death. The fact is well known that this circumstance occurs more frequently in some diseases than in others—apoplexy for instance. The action of the oxygen of the external air on the skin does not produce this effect, for the surface is death-cold, while a thermometer placed in

the abdomen rises several degrees, at a period, and under circumstances, that the equilibrium of temperature between the whole mass of solids and fluids and the surrounding medium, must have taken place if no fresh heat had been generated; and before the ordinary period at which putrefaction would have commenced. From which there can be no doubt that heat must continue to be generated for some time after the cessation of the respiratory and cutaneous functions.

If Dulong and Despretz had proved by their experiments that the oxygen inspired by the animals was sufficient to account for *all* the heat produced, they would have proved too much; and my own conviction is, on considering these experiments, that they militate not in the slightest degree against the conclusion arrived at by Liebig, that the combustion of the elements of the food is the sole source of animal heat.

Liebig reaches his conclusion most satisfactorily by another route. He proves that the carbon and hydrogen consumed in the body in a given time is sufficient to account for all the heat produced.

By a careful examination, conducted on an extensive scale, of the ultimate component parts of the food of some German soldiers, it appeared, after allowing for the evacuations by urine and stool, that upon an average each man consumed 13.9 ounces of carbon daily. Every ounce of carbon, according to Despretz, evolves during its combustion as much heat as is equivalent to 14207°. The 13.9 ounces of carbon, therefore, on being converted into carbonic acid gas, must give out 187477.3 degrees of heat. This quantity of heat would raise an ounce of water from 32° to 32° + 197477.3 = 197506.3°, which would heat 370 lbs. of water to 98.3°, or convert 24 lbs. of water into vapour.

Supposing that only 3 lbs. of water are vapourised by the skin and lungs in twenty-four hours, there will remain, after deducting the heat thus expended, 146380.4° from the combustion of the 13.9 ounces of carbon conveyed into the system with the food; which amount of heat must, upon an average, have been available daily for heating the expired air and the excrementitious matters, for supporting the radiation of the body, and for maintaining its temperature at 98.3°.

There remains the heat evolved by the combustion of the hydrogen of the food during its conversion into water, which for equal weight is much greater than that arising from the combustion of the carbon. We have, also, to consider that the specific heat of the bones, of fat, and of the organs generally, is less than that of water, and "no doubt can be entertained that when all the concomitant circumstances are included in the calculation, the heat evolved in the process of combustion to which the food is subjected in the body, is amply sufficient to explain the constant temperature of the body, as well as the evaporation from the skin and lungs."

Weighing these results maturely, notwithstanding the seeming discrepancy above indicated, I think the conviction is irresistible, to use the words of Dr. Thomson, as employed in his recent work on Animal Chemistry, "that the whole animal heat evolved is owing to the conversion of the oxygen gas absorbed into carbonic acid gas and water during the circulation." It should be noticed that this combustion leaves a caput mortuum in the shape of certain oxidised substances in the urine; and, as before insisted upon, the true physiological doctrine must be, that the combustion of the elements of the food is the invariable source of the heat extricated in the animal body.

Nothing can be of greater importance in the application of physiology to practical medicine, if this doctrine be true, than that the mind should arrive at a thorough conviction of that truth. Daily experience proves that a vacillating practice is the invariable concomitant of instability in our principles, whether of pathology or of physiology. Among other highly important relations, those which subsist between the food we consume and the air we breathe are involved in this doctrine. The maintenance of animal heat is one condition of life, but how shall we proceed with any degree of confidence, to regulate the nature and kind of exercise which our patients are to take, their food and drink, their hours of sleep and watching, and their habits generally,—how shall we be able to estimate variations in the temperature of the body, and modifications in its power of resisting external influences, if we know not the simplest relations subsisting among the operations in the animal economy, to which the general term is applied, of the functions of life?

Of our author's doctrine I now place before my readers his illustrations derived from the observations of physiologists generally. These observations had previously created in the medical mind a vague notion of the true relation that exists between the ingesta of food and the evolution of heat, but they have never before been urged in proof of the position, that the combustion of the elements of food is the sole source of heat in animals. The reason for this is plain—there were anomalies, seemingly irreconcileable according to existing facts and theories. Liebig's work annuls the anomalies and explains the facts with beautiful simplicity. The following illustrations convey to the mind, also, a just notion of

The extensive practical application of this subject.

According to Liebig's theory, then, the amount of heat liberated by an animal must increase or diminish with the quantity of

oxygen introduced, in equal times, into the system, chiefly by respiration, the size of the body being equal. Species or individuals which consume most oxygen should possess the highest temperature.

(1.) In those animals only which possess lungs, and consume large quantities of oxygen, is the temperature of the body quite independent of the surrounding medium.

(2.) A child breathes quicker, and in proportion to its bulk consumes more oxygen, than an adult. Its temperature is 102°, that of the adult being 99.5°.

(3.) Birds consume more oxygen than quadrupeds. Their temperature is (104° to 105.4°) as compared with (98.4° to 100.4°).

(4.) Man, quadrupeds, and birds consume much more oxygen than fishes or amphibia, whose temperature does not extend beyond 2.7° to 3.6° above that of the medium in which they live.

But the temperature of man and of warm-blooded animals is invariably the same in all climates. Now the body of an animal must cool more rapidly in a cold than in a warm atmosphere. The loss of heat must be greater, and the heat produced must be greater. In cold climates, accordingly, more oxygen is inspired in a ratio equivalent to the increased loss of heat; for, in the first place, the same bulk of air being inspired, it contains more oxygen by virtue of its increased density; and, secondly, more exercise being taken, the number of inspirations is increased. With the increased consumption of oxygen an increased quantity of carbonic acid gas is exhaled, hence the quantity of combustible material received into the system must also be increased. The enormous appetite possessed by certain tribes inhabiting cold regions, and constantly exposed to the cold, and their consumption of large quantities of hydro-carbonaceous materials, is thus explained. The carbon and hydrogen suffices only to keep up the equilibrium between the external temperature and that of their bodies. In equatorial regions the converse is observed. Little heat being required to be produced there is but little appetite, and the food; if the dictates of nature be observed, contains but a small proportion of carbon and of hydrogen.

As respects season, again, in our own climate, for instance, by taking exercise in winter in a cold atmosphere the respiration increases. The amount of oxygen inspired must accordingly increase. The necessity for food containing carbon and hydrogen increases in the same ratio. The capability of resisting the effects of the external cold is equivalent to these two circumstances, absorption of oxygen and consumption of food. A starving man is soon frozen to death. Cold seasons and cold climates urge man to laborious efforts and increased respiration. In hot climates and hot seasons the converse holds good.

Regarding many of our habits of life we meet with numerous illustrations of the same principle. The more warmly we clothe ourselves, and the more we confine ourselves to heated apartments, the less urgent is the appetite for food, because the loss of heat by cooling, and consequently the amount of heat to be supplied by the food is diminished. Under exposure to long-continued and severe cold, with corresponding exercise, even in this climate, a large consumption of food is consistent with health. The cooling of the body, however produced, increases the amount of food necessary. Mere exposure to the open air on the deck of a vessel increases the loss of heat, and with it the appetite. Drinking cold water, which must be given off from the body at its own temperature, 98.5°, must tend to cool the body. It increases the appetite, and persons of weak constitution find it necessary, by continued exercise, to supply oxygen to the system, for the purpose of restoring the heat abstracted by the cold water. Conversely, the habit of drinking hot fluids diminishes the appetite. Liebig establishes and illustrates his doctrine not only by these well-known facts, but by the following

Deeply-interesting Physiological Phenomena.

In the progress of starvation oxygen continues to be inspired at the rate, perhaps, of thirty-two ounces daily, no fresh fuel being introduced into the system, and accordingly the body itself must be burned. The first effect of this is the disappearance of fat, a compound of carbon and hydrogen. These elements cannot be traced either in the urine or in the scanty fæces of a starving individual, they must be given off as carbonic acid gas and water, and must accordingly be a means of keeping up the animal temperature. Not the slightest particle of the oxygen received into the system is given out again in its simple form; the whole of it, therefore, must combine with the constituent particles of the body. After the fat, by degrees, all the solids capable of being dissolved disappear. The muscles are consumed, and ultimately the brain and nervous structures begin to undergo oxidation. The attack on the last-mentioned parts is characterised by delirium and other nervous symptoms. Every part of the body, bones excepted, enter more or less into combination with oxygen. The effect of all this is to maintain animal heat, without which, as we have seen, animal life cannot continue for any length of time; the lamp burns as long as the fuel lasts, and the true cause of death is the oxygen inspired.

The same thing is seen in hybernating animals. During the winter-sleep the oxygen consumes all those substances which are capable of entering into combination with it. This combustion must tend to maintain the animal temperature.

When there is a deficiency of oxygen, as compared with the amount of food, those elements of the food which do not undergo the process of slow combustion, are given off in unburned in the excrements. Oxygen being absorbed, not only by the lungs, but in a minor degree by the cutaneous surface, from the first moment that the function of the skin or lungs is interrupted or disturbed, compounds rich in carbon appear in the urine, which fluid acquires a brown colour. The cutaneous secretions also acquire peculiar characters where the action of oxygen is prevented, as in the arm-pits and soles of the feet; they acquire a peculiar odour and become carbonaceous.

This, then, is the relation which subsists between the oxygen inspired by an animal and the combustible elements of its food. The one is the supporter, the other the fuel; the result, as in ordinary combustion, is heat. Animal heat has no other source. I proceed to state briefly some most interesting details appertaining to

Liebig's Theory of Respiration in its wider Application.

The first relates to the laws of the absorption of oxygen in the lungs, and the transmission of this important element to and from every part of the system.

Liebig recals our attention to the fact that for every volume of oxygen absorbed an equal volume of carbonic acid gas is in most cases given out. He furnishes evidence to prove that the oxygen is not absorbed by the serum, but by the globules of the blood, and that the carbonic acid gas is not separated from the serum, but from the globules also. He shows that the red globules have the power of combining with gases, and that they contain an oxidised compound of iron as their peculiar constituent.

The oxides of iron are possessed of a remarkable peculiarity. The compounds of the protoxide have the property of depriving other oxidised compounds of oxygen; the compounds of the peroxide part with oxygen (under circumstances) with the utmost facility. Hydrated peroxide in contact with organic matter destitute of sulphur is converted into the carbonate of the protoxide; the carbonate of the protoxide in contact with water and oxygen becomes decomposed, its carbonic acid being extricated, and its iron passing into the state of hydrated peroxide.

Applying these facts to the blood, Liebig suggests that the globules of arterial blood contain a compound of iron saturated with oxygen, which, during the passage of the blood through the capillaries, passes by reduction or loss of oxygen into a compound of iron containing less oxygen, and this compound which loses its oxygen, has the power of absorbing or combining with carbonic acid gas. The globules thus altered by the loss of oxygen and the gain of carbonic acid, reach the lungs as those of venous blood, where they recover the oxygen they had lost, and for every volume of oxygen absorbed a volume of carbonic acid gas is given off.

It will be observed that it is not stated simply that the globules of venous blood are composed of carbonated protoxide of iron, and those of arterial blood of hydrated peroxide of iron, for it is a fact, that when the former compound passes into the latter, for every volume of oxygen absorbed *four* volumes of carbonic acid are given off, whereas in respiration the volumes are as one to one.

Liebig takes a still closer view of these circumstances.

According to the best calculations which he believes can be made, rather less than twelve grains (35.8 cubic inches) of oxygen gas are added to the blood per minute. In the same period 10 lbs. of blood pass through the lungs; 10 lbs. of arterial blood contain 61.54 grains of peroxide of iron, and the same quantity of venous blood 55.14 grains of protoxide of iron.

The protoxide of iron of the venous blood therefore, take up in the lungs 6.40 grains of oxygen per minute in passing into the peroxide, and since twelve grains of oxygen are absorbed in the same time, 5.6 grains of this element must combine with the other constituents of the blood in its passage through the lungs.

Thus, all the constituents of the blood become saturated with oxygen in their passage through the lungs. This is seen by the circumstance that the blood retains its florid colour until it reaches the capillary system. Animal fibrine contains a greater quantity of oxygen than proteine, albumen, or caseine. Other substances also are oxidised in their passage through the lungs. So that, according to this view of the case, two processes of oxidation are going on in the animal organism; one in the lungs, which, in spite of evaporation and the cooling process, keeps up the temperature of these organs; the other throughout the whole body by means of the oxygen conveyed by the iron of the globules of the blood.

The 55.14 grains of protoxide of iron combine with 73 cubic inches of carbonic acid gas; therefore the iron of the blood is sufficient to furnish the means of carrying or transporting more than twice the bulk of carbonic acid than can be possibly formed by the oxygen absorbed in the lungs, or than can accordingly be given out by the lungs. The living blood is never in a state of saturation with carbonic acid gas as shown by the absorption of this gas under a variety of circumstances, but the compound formed with carbonic acid, as before intimated, is

destroyed by the oxygen in the passage of the blood through the lungs.

Such is the ingenious theory suggested by Liebig to explain the absorption and exhalation of the gases in the lungs and in the capillaries, and their transmission to and from the extreme arcs of the circulation. I say "suggested," although it is founded upon facts determined by chemists. If this should turn out to be the true interpretation of this part of the respiratory process, then, indeed, will chemistry have achieved a triumph. If chemistry can accomplish this much, then may we not despair of attaining an insight of the most recondite processes of the animal economy. I am not prepared to offer one objection, put as the theory is, in a problematical way, by the author. On the contrary, by admitting its validity, an explanation will be found of several phenomena hitherto inexplicable. If any fallacy exists as to the facts upon which this part of the theory is based, it remains for chemists to determine by experiment that such is the case.

Having thus attained a clear conception of the manner in which the elementary oxygen is received into the system and conveyed with arterial blood to all the vital parts, and the manner in which it is carried out of the system after supporting combustion, which is the source of that heat, without which the manifestations of life cannot continue, inquiry is naturally invited to the intermediate changes which the combustible elements of the food undergo from their digestion to their final elimination as water and carbonic acid gas. A few facts only appertaining to this part of the subject are known, and the first question that suggests itself relates to

The Use of the Bile.

In Paper VIII. (page 613), which treats of this substance, a compound of an organic combustible with soda, the probability was shown that its quantity is considerable, and that, as a general law of the animal economy, its constituents are reabsorbed from the intestinal canal after admixture with the contents of the latter, and the probable performance by them of some important office in digestion. Considerations relating to nutrition (Papers VI. and VII.) indicate that the bile cannot have any share in the production of "blood," that is to say, the protenised compounds of blood; nor can it serve for the nutrition of the tissues. In carnivora, in the winter-sleep of hybernating animals, and in starvation, it can be formed only from compounds separated from the tissues, and broken up by the chemical forces. If it were intended for excretion, it would be found more or less altered, together with its soda, in the excretions; but disappearing entirely in the animal economy, it must yield to the action of oxygen, hence it must serve for the production of animal heat.

These facts, and the reasoning founded upon them, have led Liebig to the conclusion that the function of the bile is to support respiration and produce animal heat by presenting carbon and hydrogen in a very soluble form to the oxygen of arterial blood. Appertaining to the subject of respiration again, is that of

The Use of the Fat.

In illustration of another point, it has already been stated that in a starving man who continues to inspire oxygen, and must have fuel for consumption, the fat is the first part of the body which becomes absorbed. During the formation of fat (Paper VII., page 528,) oxygen is given out, and thus the process itself is a source of animal heat; but fat is deposited only when there is a superabundance of non-azotised food as compared with the oxygen inspired by an animal; thus its deposition acts as it were as a substitute for free respiration in the production of heat. When, on the contrary, too great a quantity of oxygen is inspired, as compared with the non-azotised food, fat is absorbed and consumed, and thus it becomes a substitute for food in the production of animal heat. When an animal is fattening (so far as the fat is produced in the body), the deposition of fat assists to maintain his temperature by liberating oxygen from the blood; when an animal is starving, the absorption of fat assists to maintain his temperature by supplying carbon and hydrogen for combustion; thus is the absorption and deposition of fat closely allied to the process of respiration.

The nitrogenised constituents of the food of man nourish his vital structures; the non-nitrogenised constituents supply fuel for the production of heat, be it directly or indirectly, through the bile and fat; hence the philosophy of Liebig's distinction of the substances of which food is composed into "*plastic elements of nutrition*," and "*elements of respiration*."

APPLICATIONS TO WOUNDS.

M. Desmarest, reasoning on the principle that due evaporation is a condition essential to the preservation of meats, conceives that whatever means tend to facilitate this process on wounded surfaces will render their cicatrisation so much the more rapid. In pursuance of this, he advises that charpie and perfectly dry linen should in general be the only applications to wounds; or that if a more active desiccatory agent be required, lightly calcined clay, either directly sprinkled or inclosed in small muslin bags, should be applied over the abraded surface. The last-named substance, M. Desmarest says is quite equal in value as a styptic to agaric, cobweb, or nitrate of silver, and he has used it with great success for stopping the bleedings from leech-bites and cuts of no great magnitude.—*L'Experience.*

CURIOUS CASE OF PERITONITIS, WITH EXTENSIVE DISEASE OF THE LIVER AND OTHER VISCERA.

By M. D. Thompson, Esq., M.R.C.S.L., Stalybridge.

On the 7th of December ult. I visited J. I., an engineer, ætat. 34, of low stature, and stout make, when he was restless, complaining of excessive thirst, unbearable itching of his body, and a violent pain attacking him, by paroxysms, in the right hypochondriac region. The pain was not affected by respiration or pressure. His breathing was quick and sibilant; his pulse quick, strong, and rebounding; his skin excessively hot and red, being interspersed with bullæ; his tongue moist and red, having its papillæ considerably elongated. Bleed, in a full stream, to fainting.—℞ *Jalap*, half a drachm; *chloride of mercury*, eight grains; *simple syrup*, sufficient to make the mass into two boluses, of which take one immediately, and the other in two hours.—℞ *Carbonate of magnesia*, one drachm; *sulphate of magnesia*, six drachms; *ipecacuanha*, twelve grains; *spearmint-water*, five ounces and a half; *compound tincture of rhubarb*, half an ounce. Of this mixture take two spoonfuls every four hours.

On the 8th he was apparently in a state of convalescence, nevertheless his bowels had not been evacuated.—℞ *Castor oil, spearmint-water*, of each one ounce. Make a draught, to be taken every four hours.

On the 9th his gums were swollen, apparently affected by the calomel; otherwise he was doing well.—℞ *Castor oil, spearmint-water*, of each one ounce. Make a draught, to be taken three times a-day.

On the 10th he was discharged.

On the 18th he applied to me, at my own surgery, when he stated that he had had a relapse on the 12th, which commenced with a violent paroxysm of pain in his abdomen, immediately succeeded by a swelling of it.

He was much exhausted and emaciated; his countenance was anxious and sunk; his abdomen very much enlarged, yielding on percussion a dull short sound, and an evident impulse of fluctuation; nevertheless, on pressing it he manifested no pain. —℞ *Bichloride of mercury*, one grain; *compound tincture of rhubarb*, one ounce and a half. Take one small spoonful of this mixture three times a-day.

On the 20th his exhaustion had very much increased; his abdomen considerably enlarged, yielding on percussion, in the epigastric region, a clear, prolonged resonance, and in the other regions the same resonance as previously described; his bowels had not been evacuated during the last two or three days; his urine being secreted in very small quantities. Hot turpentine was directed to be applied to the external surface of the abdomen, and a turpentine enema to be administered every fourth hour.

On the 21st his bowels had been copiously evacuated; he had paroxysms of pain in his abdomen, and tenesmus; in other respects he manifested no considerable alteration.—℞ *Chloride of mercury*, one grain; *opium*, half a grain. Make a pill, to be taken every four hours.—℞ *Sesquicarbonate of ammonia*, half a drachm; *spearmint-water*, five ounces; *sweet spirits of nitre*, and *compound tincture of cardamoms*, of each half an ounce. Take two spoonfuls of this mixture every four hours.

From this period the case progressed unfavourably, until the 2nd inst., when his abdomen was considerably larger than the abdomen of a female at the full period of utero-gestation, pressing the hypochondria very considerably upwards and outwards, having its superficies unusually thin, containing numerous enlarged anastomosing veins, engorged with livid blood, yielding on percussion, in its upper regions, as far as the umbilicus, a distinct prolonged resonance, and inferiorly, in its lower regions, a short dull resonance, and a fluctuating impulse; his countenance was awfully attenuated, pale, and sunk; his breathing was slow and short, and performed at long intervals; the impulse of the heart and pulse was very slow and weak; the heat of the surface of the body was nearly natural; the intellect was perfect, and he most urgently desired that his abdomen might be somewhat emptied, to relieve him of a disagreeable sensation of fulness. Accordingly, as a last resource, paracentesis abdominis was performed, and a gallon of fluid, resembling bilious stercoraceous matter, mixed with water, possessing no odour, was evacuated, which, on being tested with nitric and muriatic acids, indicated the presence of the colouring matter of bile.

Previously to performing the operation he discharged his urine, which was submitted for analysis to John Davies, Esq., lecturer on chemistry, &c., at the Royal Medical Institution, Pine-street, Manchester, and through whose kindness, in immediately instituting the process, I am now enabled to make public the results of the analysis, according to his own letter on the subject:—

"10, Quay-street, Manchester,
Jan. 7th, 1843.

"Sir,—I have laid aside some other analyses in order to complete your's this week. The urine contains a little free acid and some phosphate of lime. The principal and characteristic constituent is urea, of which it contains a prodigious quantity, greater considerably than I have ever previously seen, and apparently giving to the case

much peculiarity. I am, Sir, your very obedient servant,

"JOHN DAVIES.

"To M. D. Thompson, Esq."

Subsequently to the operation reaction occurred; his pulse increased in frequency and fulness; his countenance improved; his breathing became more natural, and he expressed himself free from pain; but such were only temporary, as he died on the following morning about twelve o'clock.

Post-mortem Inspection.

The abdomen contained nearly a gallon of fluid, resembling the fluid previously removed from it. The omentum, the whole of the intestines, and the liver, were firmly agglutinated together. The omentum had generally become very hard and thick. The whole of the intestines were greatly distended with flatus. The peritoneal coat of the liver was firmly adherent to the parietal peritoneal coat of the diaphragm. Nearly the whole of the parenchymatous structure of the posterior thick portion of the right lobe of the liver was absent; in which situation was a vomica, having its parietes almost of a cartilaginous hardness, and apparently being principally formed of the tunics of the liver. From the vomica, opposite to the coronary ligament of the liver, a sinus commenced, extending itself obliquely downwards, outwards, and forwards, towards the left side, traversing between the peritoneum and pancreas as far as the upper portion of the duodenum, thence insinuating itself between the peritoneal laminæ of the transverse mesocolon, along which course it traversed as far as midway between the origin and the insertion (if so they may be called) of the transverse mesocolon; thence traversing through the anterior peritoneal layer of the mesocolon, which was generally and firmly adherent to the posterior peritoneal layer of the gastrocolic omentum; subsequently through both peritoneal layers of the gastrocolic omentum, and terminating in a large ragged, open mouth, on the anterior surface of that membrane, midway between the larger curvature of the stomach and the transverse arch of the colon. The sinus was empty, and appeared to have contained a pint of fluid, principally bile, which was effused into the abdominal cavity. Apparently the remaining portion of the liver was perfectly healthy. The gall-bladder was healthy, and contained bile, being of the proper consistence, but unusually dark-coloured. He had only one kidney, which was situated in the left loin, and it was apparently perfectly healthy. The remaining portion of the abdominal viscera presented nothing remarkable.

The viscera of the other cavities of the body we had not permission to examine.

History.—Previously to 1840 he had followed the occupation of spinner, and from 1840 until the 7th ult., that of engineer. In 1829 he had fever, from which he was not considered to be in a state of convalescence in a less period than eight weeks; subsequently he apparently enjoyed good health until the 7th ult., although occasionally he complained of pains shooting through the right hypochondrium, during the period he attended to the last employment, an employment the most laborious, wheeling daily and almost constantly large heavy measures of coal, in a wheelbarrow, up the ascent of an inclined plank, from a yard exposed to the open atmosphere, to the very hot fires in an engine-house, and thereby subjecting himself to sudden and frequent transitions from heat to cold, when perspiring profusely, in which condition he was frequently addicted to drink large quantities of cold ale; nevertheless he never was considered to be disordered except from drunkenness.

Remarks.—From the preceding facts the following practical inferences may be deduced:—

First. That pain is not indispensably necessary to constitute chronic or subacute peritonitis.

Second. That paroxysms of pain, unaffected by pressure, do not invariably indicate the non-existence of inflammation.

Third. That where ascites exists the necessity is imperative of exploring the whole of the abdominal regions by percussion, to ascertain whether or not it is complicated with tympanites intestinales.

Fourth. That a considerable extent of disease may exist in the liver and peritoneum without occasioning corresponding characteristic symptoms.

Fifth. That when a sudden paroxysm of pain occurs in the abdomen of a person apparently in good health, speedily followed by a swelling of it, extreme emaciation and exhaustion, and ascites, a rupture of some internal part may be suspected.

With reference to the urine I shall at present offer no remark.

Stalybridge, Jan. 10, 1843.

PHYSIOLOGY OF MENSTRUATION.

DR. RACIBORSKI has communicated to the French Academy of Sciences the following conclusions which he has drawn from his researches on this subject. Very intimate relations exist between the catamenia and the Graafian vesicles; menstruation begins at the period when these are fully developed, and ceases when they are effete (detruits). At each menstrual period one of these vesicles swells and projects from the surface of the ovary, from which it escapes by the rupture of its sac, usually about the end of the menstrual flow, without male congress or other sexual excitement. The vesicle which thus escapes has anatomical characters precisely similar to those of the

corpus luteum supposed to be formed after conception. Diseases arrest the development of the vesicles, and it is in this arrest that the true cause of amenorrhœa is in many cases to be sought; for the catamenial flow is the result of the sanguineous congestion of the internal generative organs, by which the development of the vesicle in its highest degree is accompanied. The state of the ovaries is so intimately connected with the general health of an individual, that the internal appearance of these organs after death is said by Raciborski to be alone adequate to determine whether the patient has sunk under an acute or a chronic disease, or whether she has latterly menstruated regularly.

Phenomena analogous to those specified above as occurring in the human female at the menstrual period, are remarked also in animals during their rutting season. Among these the ovarian vesicles are found to increase gradually in size during the interval between the rutting epochs; and they ultimately escape altogether at these epochs without congress of the male. The spontaneous detachment of the human ovum at the end of the menstrual epoch naturally renders that period the most favourable for impregnation, and readily yields a reason for the fact that conception is most commonly referred to that epoch by pregnant women. "Of 15 women (says Dr. R.) who specified accurately the period of their latest menstruation, as well as the dates of the connubial act, 5 evidently conceived from coitus taking place from two to four days previous to the period at which the catamenia was due. In 7, conception dated from coitus occurring two or three days after menstruation; in 2, it took place at the actual period of the catamenia; and in one only so long as ten days after the latter had disappeared.

Considered with respect to her generative function, woman holds a place intermediate between rutting animals, which are capable of impregnation only at fixed seasons of the year, and those animals in which a coitus only is required to produce impregnation at any season. She, however, approaches much nearer in point of this analogy to the former class, her power of reproduction being infinitely the more active at her menstrual periods, to which the rutting time in brutes bears a strict physiological resemblance.—*Gaz. des Hôpitaux.*

*** There is much that is new, rational, and striking in the above views, and we shall keep an eye upon any future observations that may be made by Dr. Raciborski on this subject.

Analysis of a Polypus.—M. Girardin, of Rouen, has found a vaginal polypus which he submitted to a chemical examination, to consist chiefly of fibrin, with small quantities of fatty matter, hematosine, and phosphate of lime.

CASE OF ABSCESS IN THE WALLS OF THE UTERUS, COMMUNICATING WITH THE RECTUM.

Related at the Westminster Medical Society, Dec. 3, 1842, by Dr. Frederic Bird.

Mrs. G., aged 37, had, previously to the last three years, enjoyed general good health, menstruating regularly. At this date she married, and was soon afterwards attacked with acute deep-seated pain in the hypogastric region, radiating to all parts of the pelvis, and increased by micturition and defecation. These symptoms were associated with general constitutional disturbance, and, in fact, with all the ordinary symptoms of inflammation affecting the uterus. She passed through the usual forms of treatment, and although the more urgent symptoms were mitigated, yet she continued to suffer during the three following months from occasional pain in the region of the uterus, always produced by attempts at expelling the contents of the bladder or rectum, the discharge of fæces being also sometimes effected with great difficulty. An internal examination made at this period detected the uterus lower in the vagina than usual; there existed marked enlargement of that organ, the chief increase in size being found to occupy the posterior wall; the os and cervix uteri were painful to the touch and tumid.

Shortly after the vaginal examination had been made about half an ounce of pus suddenly escaped from the rectum, and she experienced immediate relief from her former symptoms. She now became the subject of diarrhœa, generally passing from six to eight evacuations daily, each of which contained more or less purulent matter; pain in micturition was no longer felt, but she invariably suffered greatly when passing motions. The diarrhœa could not be arrested by any of the remedies employed; her general health, nevertheless, slowly improved, and she went into the country, where she remained during the succeeding two years, little or no variation in her symptoms having occurred. The diarrhœa, and with it the discharge of pus from the rectum, continued; on some occasions more than a pint of pus has been thus evacuated during twenty-four hours, and she observed that whenever the pus failed to be discharged so freely as usual the local pain became aggravated. During the whole of this period menstruation had been very irregular, generally occurring at intervals of eight or nine days, accompanied by much lumbar pain and the passage of coagula.

After the lapse of the time mentioned she again applied to Dr. Bird, suffering from nearly all her previous symptoms, and, in addition to them, profuse menorrhagia; the

pain in the region of the uterus was extremely acute, increased by the passage of the fæces and by pressure on the lower part of the abdomen, to which became added a neuralgic condition of the genital organs, the slightest pressure upon which produced extreme suffering; so great was the pain thus excited that she was accustomed to employ a mechanical contrivance to prevent the bedclothes from touching the pubes. A vaginal examination was, with much difficulty and pain, again made; the uterus was found to be nearly in the same state as before, excepting that it had become quite immoveable, appearing as if impacted in the pelvis, just as may be observed in some forms of malignant disease affecting that organ. No benefit resulted from medical treatment, occasional relief only being afforded by large doses of opium and the external application of belladonna.

She continued to suffer from frequent discharges of blood from the vagina, and from all her former symptoms, until the lapse of six weeks, when she sank exhausted by the extreme suffering produced by her disease.

A post-mortem examination was made twenty-four hours after death. On laying open the abdomen the, omentum, small intestines, and all the pelvic viscera, were found agglutinated together by peritoneal adhesions of old date. On raising the uterus it was seen to be firmly attached by its upper and posterior portion to the rectum; it presented an irregular form, having the fundus enlarged to about thrice its natural size. A longitudinal section showed this enlargement to have been produced by an abscess seated in the substance of the wall of the fundus uteri, the cavity of which contained about an ounce of dark thick pus; the walls of the abscess varied in thickness from one to three-quarters of an inch, the thinnest portion being nearest to the cavity of the uterus. A communication by means of a short sinus could be traced passing from the cavity of the abscess to the adherent portion of the rectum, and opening into that intestine by an aperture sufficiently large to admit of the passage of a thick probe, and evidently of old formation. No communication existed between the uterine cavity and that of the abscess. The os and cervix presented no evidence of malignant disease. The Fallopian tubes and ovaries were adherent to the uterus, and could with difficulty be distinguished. The uterus had never been impregnated.

He (Dr. B.) had been induced to bring the case before the notice of the society chiefly from the rarity of such forms of disease; very few cases had as yet been described, and those which he had hitherto met with in the works of Madame de Boivin and others were complicated with carcinoma or other malignant disease of the uterus. In the instance he had related no evidence of such disease existed, but it was clearly a case of inflammation of the substance of the uterus terminating in the formation of abscess. It was difficult to account for the neuralgic state of the generative organs, unless the immoveable and apparently impacted condition of the uterus might be received as sufficient to produce such an effect by mechanically pressing upon the surrounding nerves, an idea favoured by the fact that at the earlier period of the case, when the uterus was not thus fixed, the pain was limited to that viscus and did not extend to the external organs.

*** The preparation was exhibited to the society, where Dr. Chowne stated that he had never seen a similar case. The uterus and its appendages, he said, were in that conglomerated state which is usually observed in women who had led an irregular life, and which was, probably, in them, dependent upon early and excessive excitement. This adhesion of the ovaries, Fallopian tubes, &c., in prostitutes, had been noticed also in France. Perhaps, he added, in Dr. Bird's case, this condition might have been the result of the disease under which the patient laboured. On the same occasion another member of the society, Dr. Reid, said that he also was unacquainted with the record of any similar case, excepting those related by Madame Boivin, and thought the disease must be a remarkably rare one.

DETECTION OF ARSENIC IN THE EXCRETIONS.

An apothecary of Dresden, named Meurer, had entertained doubts as to the truth of the assertion that urine is the only excretion in which arsenic is to be detected after having been swallowed, and accordingly experimented on the perspiration, nasal mucus, and fæces of horses, to which the substance had been purposely administered. In the mucus and perspired matter he has hitherto discovered no trace of the poison; but this circumstance he attributes to his having obtained these excretions in small quantities only. It was otherwise with the fæces, in which arsenious acid was proved to be present, not in a state of mechanical mixture, as if it had passed through the stomach and small intestine unchanged, but in intimate combination with the fæcal matter. According to Meurer the fæces possess an advantage over the urine for the researches of the chemist or medical jurist, in cases where arsenic has been taken, inasmuch as the poison may be detected in them after a greater lapse of time. Traces of arsenic are not generally demonstrable in the urine for more than four days after its administration; in the fæces they may be proved to exist for at least twenty-four hours longer with facility. —*Wochensch. fur die gesammte Heilkund.*)

CASES EXEMPLIFYING THE

SIMULTANEOUS OCCURRENCE

OF

VARIOLA AND VARIOLA VACCINA.

Related at the Westminster Medical Society, Dec. 3, 1842.

By CHARLES BROOKE, Esq.

THE author stated that his object in bringing before the society these cases was to show, first, that the two diseases, variola and var. vaccina, might occur simultaneously (a fact not generally admitted), the effects of each disease modifying the influence of the other; and, secondly, that in a series of three cases, occurring under precisely similar circumstances, the intensity of one disease bore an inverse ratio to that of the other.

The first case was that of a girl aged five years, whom he found labouring under variola in a severe and subsequently confluent form, the period of the eruption being about the fifth or sixth day. A boy, aged three years, had been up to this time sleeping with his sister; as neither had been vaccinated, Mr. Brooke immediately obtained some lymph, taken on the eighth day, and vaccinated the child. There was some appearance, on the second day, of the formation of vesicles, but as they had entirely disappeared on the afternoon of the third day, some fresh lymph, in its most perfect state, was obtained on the morning of the fourth day, and the child again vaccinated. On the next day increased local action was evident at some of the points where the lymph was first inserted, as well as at some of those of the second vaccination. Both sets progressed *pari passu*, and arrived at maturity on the eleventh day after the first, and the seventh after the second insertion. The vesicles were more or less surrounded by an inflamed areola, but certainly not to such an extent as would probably have been the case if *var. vaccina* had alone been present in the system. On the eighth day a thinly scattered variolous eruption made its appearance; the pustules were never fully developed, and commenced declining when the vesicles arrived at maturity.

In this case it appeared evident that the system, being under the influence of variolous miasma, was incapable of developing the effects of the vaccine virus until a second time subjected to its action.

The cases illustrative of the second point, the comparative intensity of the two diseases, occurred in a family consisting of four children, a girl, aged nine years, another about six years, a boy about four years, and an infant of four months, who, for the sake of brevity, may be represented by *a*, *b*, *c*, *d*. On the 5th of October he was called to attend the boy *c*, who was evidently affected by one of the class of exanthemata, but the symptoms were not characteristic of any particular disease. On the afternoon of the 6th the symptoms of variola were manifest, and it appeared that none of the children had been vaccinated. Mr. Brooke, in consequence, obtained some lymph on the next morning, the 7th; but it was not, however, in its most efficient condition, being from vesicles of the tenth day. In consequence, however, of some unmeaning scruples on the part of the father, the vaccination of the other children was not permitted. On the next day, the 8th, the fears of the mother overcame the father's scruples, and Mr. Brooke happening to have the lymph on glasses in his pocket, was requested to vaccinate the other three children, *a*, *b*, and *d*. By this time, however, the children were probably influenced by variola, and the efficacy of the vaccine virus was impaired; from one or both of these causes the vaccination was ineffectual, and on the 12th, on which day the pustules on *c* had arrived at maturity, *a*, *b*, and *d* were revaccinated with lymph in its most perfect condition, obtained from vesicles of the eighth day, and conveyed in a moist state, in a bottle constructed for the purpose, and for which Mr. Brooke is indebted to Mr. Tomkins, the resident medical officer of the National Vaccine Institution.

This was described as being the most efficient mode of preserving lymph in a moist state, or, indeed, of preventing the escape of any fluid, such as ammonia, strong acids, &c. The bottle is closed by a ground stopper, which has a groove cut round it, at about the middle of its bearing; by this means the capillary action which goes on between the stopper and the neck of the bottle is intercepted by a ring of atmosphere, or vapour, of the contained fluid.

On the 15th *a* manifested some constitutional disturbance, which was much aggravated on the 16th, on which day *b* was also complaining. On the same day perfect vesicles appeared to be forming on *d*, with slight febrile symptoms. On the 17th *a* and *b* had much increased general fever, with derangement of the liver, *a* more severe than *b*; very slight appearance of vesicles. On the 18th vesicles were forming on *a* and *b*, and general symptoms abated, *a*, however, worse than *b*. On the 19th pustules formed on *a* and *b*, thickly covering the entire surface in *a*, and subsequently confluent on the face, but much less thickly scattered, and not at all confluent in *b*. On the same day a very minute but thickly spread eruption appeared on *d*, with very slight febrile symptoms, not more than usually occurs when the vaccine vesicles are progressing to maturity. On the 20th the pustules and vesicles were advancing in *a* and *b*, and the febrile symptoms abated; in *d* the vesicles arrived at maturity, and were surrounded by the usual inflamed

areola; the eruption had entirely disappeared. Another infant was subsequently vaccinated with lymph taken from *d*, and the disease went through its usual course without presenting any anomalous symptoms. On the 22nd, the tenth day after the effective vaccination, both the pustules and vesicles were matured, and for the succeeding few days *a* suffered severely, and *b* slightly, from the ordinary symptoms of variola. It may be here remarked, that in *a* and *b* the vesicles, though perfect in form, were not surrounded by the usual areola of inflammation; in *a* they closely resembled the pustules, both in size and appearance, but in *b* they were much larger than the pustules. On the 26th *b* was convalescent, and on the 31st *a* was convalescent, but suffering much from debility, not being able to walk across the room, or even to stand alone.

These cases, it may be remarked, were exposed to the same contagion, and subsequently to the prophylactic influence of the same virus.

The infant was completely protected from the effects of variolous contagion, unless the eruption, which appeared for a few hours only, may be considered as a slight modification of the disease, and the two eldest were decidedly though differently affected by both diseases, but in these the struggle between the two appeared to protract the general disturbance, and to delay the appearance of both beyond the usual period.

*** In reply to a question from Mr. Lavies, Mr. Brooke stated that in *a* and *b* the declining vesicles did not form as thick a crust as usual, but those of *b* approached much more nearly to the character than those of *a*. In the course of discussion Mr. Lavies remarked, that these cases were quite opposed to the doctrine that two specific diseases could not go on at one and the same time in the system. He had not seen any similar examples. He had met with cases in which vaccination was arrested by small-pox, and when this disease had run its course the vaccination had again progressed to its termination in the usual manner. Dr. Reid observed, that he had seen two cases in which cow-pox and small-pox proceeded, *pari passu* in the same child; and Dr. Gregory had seen similar instances. A curious case had lately been published in one of the journals of a woman who was delivered of a child covered with small-pox pustules. The mother had been vaccinated. She felt poorly only a few days before she was delivered. The protective power of vaccination in this case did not extend to the offspring. Mr. Wells said that he had seen two cases in which the cow-pox and small-pox progressed to maturity at one and the same time in the same patients. In one instance the variola was somewhat modified.

ON CATHETERISM OF THE EUSTACHIAN TUBES.

To the Editor of THE LANCET.

SIR,—I have nearly refrained altogether from making observations on this mode of practice, but now that the *mania* has in some degree subsided, probably the voice of common sense and reason, founded on a thorough knowledge of the anatomy of the ear, may, through the favour of your highly useful work, dispel the mist from the eyes of those professional men who have either been too indolent to trouble themselves on the subject, or too ignorant of the structural formation of this organ of sense to understand whether the above plan were likely to prove beneficial or otherwise.

First, then, as to the anatomy of the parts. The orifice of the Eustachian trumpet is nearly on a level with the internal and rising portion of the opening of the inferior meatus narium into the pharynx. This orifice of the Eustachian tube does not present a round aperture, but an oblong opening, ending at its lower part in an obtuse point, formed by a fold or wrinkle of the integuments of the pharynx; immediately behind this gutteral duct of the ear is a prominence, caused by the cartilage of the orifice which I am describing, forming another fold; and beyond that, again, is the *sacculus cæcus* of the pharynx. The two first folds extend downwards as far as the tonsils and uvula, and the latter much lower. The Eustachian tube is formed of cartilage nearly half way to the cavitas tympani, the remainder is formed of the petrous portion of the temporal bone, wherein is placed the cochlea and more internal parts of the auditory apparatus, which it is not necessary for my present purpose to notice. The Eustachian tube enters the cavitas tympani at its lower part, and it therefore becomes now proper to mention the contents of this cavity. These are, the concave membrane that closes the foramen rotundum, which aperture leads to that part of the cochlea called the scala tympani; the foramen ovale, leading to the vestibule, and closed by the base of the stapes; the malleus, attached by its lower extremity to nearly the centre of the membrana tympani, which it draws into a convex shape on the internal side, consequently concave externally; the incus, articulated with the head of the malleus, its lower part articulated with the os orbiculare, and that again with the head of the stapes; the muscle of this last-named bone; a tolerably large branch of the internal carotid artery, with another of the internal jugular vein; and at the upper part the opening to the cavernulæ of the mastoid cells. In addition to all this, the cartilaginous part of the Eustachian tube, as well as the bony part, is furnished with integuments, ramifications of the hard portion of

the auditory nerve, with accompanying blood-vessels, both arterial and venous. It is also proper to remark that the small end of the incus, with the small round bone and the stapes, stretch directly across the cavity of the tympanum.

Nearly one hundred and twenty years ago the subject was agitated in France, as to the possibility of cleansing the Eustachian tube by injections by an instrument passed through the mouth; but this was declared to be *impracticable* by very competent authority. In 1755, Mr. Wathen passed a catheter through the nose into the Eustachian tube, and imagined he had rendered great service to science, but it proved a failure. I repeated the process in 1816-17, and, like Wathen, abandoned it. Deleau, at Paris, who had for several years previously endeavoured to gain notoriety, adopted the plan, adding to it a large vessel of condensed air, which he allowed to pass through the catheter, and, by advertising this new method as of the greatest utility, he induced numbers of deaf persons of this nation to consult him, many of whom have since consulted me, having derived no benefit from his treatment. In 1838 he published a very plausible treatise on the subject, which, if not filled with erroneous statements, is a tissue of a much worse character.

Dr. Kramer, of Berlin, copied the methods of Deleau, and published a work in which he, without exception, condemned the treatment recommended by every author; this was translated into English, but reflected no great credit on the author, as it evinced want of chemical knowledge, deficiency of skill in the minor operations of surgery, and either a lack of anatomical accuracy or a desire to attract the popular world by statements unfounded in fact; independent of his uncourteous attempts, and shallow reasoning in his endeavours to raise himself into notoriety by depreciating every one else. As everything German is fashionable, apothecaries, &c., from Wales and Ireland, and general surgeons in London, suddenly started into full-fledged aurists, although no one who knew them would give them credit for the least knowledge of the subject.

But, adopting the plans of Kramer, they enlisted the assistance of personal friends, the charitable feelings of the benevolent, and even the aid of some class of religious dissenters, in favour of what they denominated "new modes of practice," which would be "*certain to cure every one afflicted with deafness.*" Meetings were convened, which medical men were solicited to attend, to see the wonders of the new system. Patients, also, who from the confident tone of the operator *thought* themselves better, were induced to display their oratorical powers, eulogising this plan as the greatest boon which science had ever conferred upon man, and heated by the opportunity thus afforded them, forgot all former ties of friendship, and even gratitude. As an instance, a solicitor, whose case was hopeless beyond relieving him, as I did, of a discharge of a puriform nature, under which he laboured, and whom I attended whenever he chose to call without expecting a fee of him, went to one of these meetings and volunteered an eulogium upon the plan and its professor, stigmatising aurists as incompetent pretenders (obligations to me of a private nature should have prevented such allegations), and all advertised in the newspapers, notwithstanding which he is not in the slightest degree benefited, being as deaf as he ever was, if not even worse, as I am just now authentically informed. And such is the situation of nearly, if not quite, all who fancied themselves relieved; for I have had as patients great numbers who have been thus experimented upon. To the credit of the really scientific portion of the profession, catheterism of the Eustachian tube, and the introduction of a rush of condensed air, has never found one solitary supporter among them.

I shall now proceed to show that Deleau's plan, which has been copied by Kramer and others, is only equalled in absurdity by that of a man in this country who pretended to cleanse the Eustachian tube by stopping the external auditory passage with scented cerate! and that of the more recent, *but old*, method of cutting the uvula and scarifying the tonsils, is about on a par with the practice of another professor, who touched the throat every day or so with caustic, during six months, in order to cure a polypus in the external auditory passage!

In the first place, the introduction of any substance continually, if of a hard nature, into the external auditory passage, which is formed of similar cartilage, bone, integuments, nerves, and blood-vessels as the Eustachian tube, would occasion pain, abrasion of the cutis, and ultimately inflammation and puriform discharge; and the same effect would be produced more rapidly in the Eustachian tube by the introduction of a catheter. Of this I have had recently two convincing proofs; one in a lady from India, who wrote to me in 1838-9, and this year came over to consult me. She was intercepted, however, by some friend of one of these advertising catheterists, who amused her during some months, during which time he cut her uvula and tonsils without affording her the slightest relief. She then came to me, and as she said that her throat was sore I ordered her a gargle, which brought away matter streaked with blood. Another, a lady from Liverpool, who, also, had been under a *mal-adroit* catheterist, who occasioned great pain without affording her the least relief. I gave her directions and sent her home, and as there was soreness of the throat, she also used a gargle. It is now a month ago since she left town, and I have

just heard from her that a species of matter comes away every time she uses the gargle, and I am much pleased to learn that it is less in quantity, and from the means which I have advised she is amending with regard to her sense of hearing. Thus far may be seen the injury which the introduction of a catheter is likely to produce, and which it actually did occasion in these instances.

Now, suppose that there be in the Eustachian tube, or in the cavity of the tympanum, any accumulation of a pus-like nature, or even a similar collection in the Eustachian tube, occasioned by the passing of the catheter, it would be forced by any stream of condensed air into the cavity of the tympanum, and thence some portion of it would enter the mastoid cells, and defeat the uses for which nature designed them. When persons contract what is commonly called "a cold," a considerable quantity of mucus collects in the passages of the nose; this becoming troublesome, the handkerchief is in frequent requisition, and is as improperly used by the generality of mankind, who press the nostrils together very forcibly, and then use all the force of the air from their lungs to expel the obstruction. If the Eustachian tube be pervious, the weight of air passing up and pressing against the internal side of the membrana tympani weakens its attachment to the fine end of the malleus, and by degrees occasions, from this cause alone, a species of deafness, one symptom of which is that the patient hears better in a coach, or during the continuance of any loud sound; but is a kind of deafness very difficult, if not even impossible, to cure, one reason of which is that persons once acquiring this unfortunate habit seldom or never abandon it. If, therefore, the weight of air from the lungs can and does occasion such injury, how much more will the contents of a large vessel of condensed air produce upon this part alone? The evil is not, however, confined to the membrana tympani; there is the lower branch of the incus, and its articulation with the stapes, and the concave membrane of the foramen rotundum, which are all exposed to most serious injury from a rush of a large body of condensed air among them. Even the large branch of the internal jugular might be ruptured by the process, and then death would be instantaneous, as was the case, no doubt, of Joseph Hall, upon whom you held an inquest in June 1839, as there was evidently a rupture of a blood-vessel in the cavitas tympani. If the catheter does not pass into the Eustachian tube, and the discharge of condensed air takes place in the throat, the consequence may not be so sudden, but, as in the case of Mr. Whitbread, upon whom an inquest was held in the same month and year, it was no less fatal and certain. Miss Wodehouse and the late H. Ph. Hope, Esq., both lost their lives by submitting to this system; but whether the inventor of it promulgated it to the world through some hallucination, or through charlatanism making a gross attempt upon human credulity, is not of much importance. He has certainly not evinced in his writings any knowledge of the anatomy or physiology of the ear. Deleau and his copyist, Kramer, together with his second-hand imitators, have placed themselves in a very unenviable position. They have asserted that this method of catheterism of the Eustachian tube was a certain cure for every, or nearly every, case of deafness, and with great illiberality have endeavoured to depreciate every other practitioner.

What will the patients and the *venal* portion of the press now say to this vaunted "new discovery?" it having proved, like the operation for stammering, a total failure. These cases, supposed by patients themselves and others to be "splendid cures," being as bad or worse than they ever were, proving the truth of the old medical story, "conceit will kill and conceit will cure." I am, Sir, your very obedient servant,

W. Wright, Surgeon-aurist.

Duke-street, St. James's,
Jan. 7, 1843.

SPONTANEOUS CURE OF AN ABSCESS.

A singular case is on record of a pulmonary abscess curing itself by a spontaneous discharge through a leech-bite. The subject was a young Russian female, married, though only fourteen years of age, and who was attacked by a cough, attended with lancinating pains between the fourth and sixth ribs on one side, for which leeches were applied over the seat of the pain. The symptoms, however, aggravated, and a good deal of fever supervened; while one of the leech-bites inflamed, assumed a pustulous aspect, and soon afterwards broke, and gave egress to about two ounces of pus. The discharge of this matter continued at intervals, chiefly during deep inspirations and fits of coughing, for three days, when it began to diminish, and the patient not long afterwards was fully convalescent.—*L'Experience.*

Contamination of the Fœtus.—At a recent meeting of the Westminster Medical Society, Mr. H. J. Johnson related the case of a woman who, immediately after marriage, contracted syphilis, was affected with secondary symptoms, took mercury, and got, apparently, quite well. She had, during the next ensuing years, several children, all small, but healthy, until five or six weeks after birth, when they became affected with an eruption similar to lepra or psoriasis, and died. It would appear in this case that the ovaria had been diseased originally, and that the disease had contaminated the ova in them.

TREATMENT OF ULCERS FROM LYING STILL.

To the Editor of THE LANCET.

SIR,—Observing in your last number of THE LANCET a new ointment for the cure of "ulcers from lying still," recommended by a German surgeon, I send to you the following case with reference to the same subject:—

I was called to attend a female, ætat. 22, last November, whilst suffering from an attack of acute rheumatism, accompanied with violent pain along the course of the sciatic nerve; she had already been under medical treatment for several weeks, and had been confined to her bed the whole of that time; she was greatly emaciated, and had repeated attacks of syncope daily; the lower part of the back over the sacrum was ulcerated to the extent of seven or eight inches in circumference; the pulse small and rapid, and countenance anxious. I directed the patient to lie on the side or abdomen, but she stated that the pain was so excruciating on changing her position (viz., lying on her back), that she could not possibly alter it. I was, therefore, obliged to content myself with applying pads to the dorsal region, to take off as much pressure as possible from the ulcerated surface; simple dressings were applied to the sore, and tonic treatment adopted. The ulceration, however, progressed, the integuments over the sacrum sloughed away, along with the periosteum and ligamentous structure; exfoliation of the bone likewise commenced.

The case being now desperate, and the patient still unable to change her position, I adopted the following plan—A large sheet was folded, to about the breadth of fourteen or fifteen inches, and one end was passed under the loins, and then drawn up to the top of the bed, where it was firmly fixed. The other end of the sheet was elevated so as to raise the patient several inches from the bed, to take off all pressure from the ulcer; it was likewise fixed to the top. In fact, the patient was suspended by a sling; the plan answered admirably, and the ulcer soon filled with healthy granulations, and in the course of a few weeks the patient was convalescent, and the ulcer healed. I am, Sir, yours, most obediently,

T. WILSON, M.R.C.S.L.

Hornsea, Jan. 18, 1843.

INTERMITTENT FEVER.

ABSENCE OF SPLENIC LESION.—INFLUENCE OF SULPHATE OF QUININE.—SPECIFIC INFLUENCE OF MEDICINES IN DISEASE.

THE following clinical remarks were made lately by M. Recamier at the Hôtel Dieu, Paris:—

In St. Madeleine's ward is a patient with a quotidian intermittent, in whom the febrile access regularly occurs, yet there is no appreciable lesion of the spleen. No doubt such lesions are common in intermittent fevers; and Torti, who first made known the fact, has reported cases of spontaneous rupture of the spleen during their progress. But to conclude, thence, that alterations of the spleen stand to intermittent phenomena in the relation of cause and effect, is unfounded and illogical. They have no necessary connection. We see daily intermittent neuralgia, phlegmasia, &c., without any morbid alteration of the spleen. We see, also, engorgement of the spleen last long after intermittent phenomena have disappeared, and sometimes become the cause of after serious diseases, such as ascites, for instance. On the occurrence of such diseases from such causes, sulphate of quinine is decidedly indicated, which, in large doses, is in general completely effectual in conquering these engorgements. No fact is better established than that this effect is produced by the employment of the sulphate unmixed;* and a proof of its effect may be at any time observed in the gradual diminution of the spleen, day by day, under the influence of this salt.

I may take this opportunity of remarking how greatly the action of a medicine differs according as it is or is not indicated. When substances are administered in conformity with a positive natural indication for their use, the system tolerates them to a very great extent. When, on the contrary, they are administered without being indicated, or in the face of contra-indication, they yield no therapeutic action, but exert a physiological effect more or less misplaced,—a kind of poisonous influence, in proportion to their strength, or the magnitude of their doses. Thence have arisen the lamentable results which have been lately witnessed (M. Recamier alludes to untoward events which have followed the use of large doses of quinine in the French hospitals). A rheumatic patient is poisoned with doses of sulphate of quinine, which individuals attacked by intermittent fever, chronic engorgement of the spleen, take with impunity—not to say benefit. In the latter case the *indication* exists, which does not in the former.—*Gazette des Hôpitaux.*

*** It would seem, then, from the remarks of our learned contemporary, that the sulphate of quinine, which has been so much lauded for the cure of intermittent fever, rheumatism, &c., is only adapted for exhibition when those diseases are attended with engorgement of the spleen.

* M. Piorry states that the sulphate of quinine, when rendered soluble by the addition of a little sulphuric acid, is efficacious in removing splenic engorgement in much less doses and less time than without that adjunct.—*Gaz. des Hop.*, Dec. 27.

THE FACIAL NERVE A MOTOR OF THE VELUM PALATI.

Some doubt appears to have existed among certain French physiologists as to whether the velum palati is indebted for any share of its motor power to the seventh pair of nerves, with some filaments from which it is supplied. But this question would appear to be set at rest by the following case of disease occurring lately in one of the French hospitals:—A man, twenty-five years of age, was admitted with all the symptoms of hemiplegia facialis. The left nerve of the seventh pair was evidently paralysed; the mouth was drawn to the right side; the left nostril was compressed, and the orbicular muscle was incapable of closing the left eye. The admitted functions of the fifth pair of nerves seemed also to have been interfered with; the sense of smell in the left nostril, and that of taste in the left half of the tongue, had become deadened to a certain extent; and the velum palati was observed to be drawn strongly to the right side. Leeches behind the ears, blisters, mercurial frictions, and the internal use of iodide of potassium, were employed with success. In about a month all the external symptoms of facial hemiplegia had disappeared, conjointly with which the velum palati had resumed its normal direct position. This case, reported by M. Diday in the "Gazette Med." for Dec. 27th, is, without doubt, conclusive enough of the fact that what voluntary motor power may be extended to the velum must be considered due to the seventh pair of nerves; and it gives rise to another important reflection: this is, that to pathology—with comparative anatomy—we must look as the main and legitimate guide pointed out by nature to the truth in disputed or dark points of physiology, in preference to drawing conclusions from the results of experiments on living animals, which are likely to be as erroneous as the means for attaining to them are cruel and unnatural.

PAINFUL DEAFNESS A SYMPTOM OF TUBERCLE.

A recent case occurring at the Hôtel Dieu, Paris, is narrated, in which this symptom strongly displayed itself in a young woman, in conjunction with hectic fever, cough, and the thoracic sounds and expectoration characteristic of phthisis. M. Recamier remarks, "The *painful deafness* of phthisical patients must not be confounded with the *deafness without pain*, which supervenes during the course of intense fevers, and is a symptom of only secondary importance. Painful deafness is the result of tubercles in the middle ear itself. It often takes place with a discharge from the ear previously to the appearance of pulmonary phthisis; and it is remarkable that in general the ear affected, and the lung afterwards the chief seat of tubercles, are on the same side. The tendency to disease may confine itself to the ear, as in the following case:—A youth, of a seemingly athletic frame and good constitution, suffered from intense pains in the head and in the posterior muscles of the neck. He was treated for meningitis, which he may have been suffering from to a certain extent, but the progress of his complaint showed that it was not meningitis alone; violent pains in the ears, with deafness, ensued, followed by delirium and death. On examination a tuberculous mass was found in the middle ear (*ayant envahie tout le rocher*), but none elsewhere; no trace of tubercle in the lungs.

Here, then, the tuberculous diathesis spent itself solely on the formation of a tubercle in the ear; this tubercle having inflamed, the inflammation was propagated to the meninges and brain, at the base of which some softening and suppuration was perceptible. Tubercles will often exist in unusual situations, and their diagnosis is then one of extreme difficulty, and often impossible. But although ordinary deafness may be a symptom of little importance, painful deafness is one that demands our attention, as of some value in our diagnosis. He might have added our prognosis also, if the relation observed between the diseased ear and the lung of the same side be at all constant. The observation respecting painful deafness, as a symptom of phthisis, is, however, not new; it was recorded by Hippocrates, and in later days by Baglivi, though in more modern times it has not received the attention it deserves.—*Gaz. des Hôpitaux*, Dec. 29.

IODIDE OF POTASSIUM.—ITS EFFECTS ON THE SYSTEM.

The following observations on this subject are extracted from a memoir on the pathognomonic action of the iodide, recently published in Paris, by M. Ricord:—

Of all the tissues of the human body, the skin is decidedly that which is most susceptible of the action of this medicament. Eczema, herpes, erythema, impetigo, rupia, an affection simulating acne, &c., are common in patients submitted to its influence; and, in short, there is scarcely any kind of cutaneous disease which, in different idiosyncrasies, may not be excited by iodide of potassium, though these effects may be mostly made to disappear by the discontinuance of the remedy.

Less decided, but still very strongly marked, is the influence of the iodide on the digestive organs. In most cases the functions of the stomach, &c., are roused into great activity: patients gain appetite and fat. Occasionally, indeed, this effect passes a salutary limit, and the appetite becomes

ravenous, or acute pains are felt at the cardiac extremity of the stomach. These pains, however, seem to be wholly gastralgic; they are not attended with thirst, furred tongue, fever, &c., or increased on pressure, or during the period of digestion; and in only a few individuals does a veritable inflammation of the stomach or intestines occur, causing vomiting, diarrhœa, &c.

Salivation is a common effect, and sometimes the quantity of saliva secreted is equal to that in the highest degree of mercurial salivation, but it appears to be of a different character. M. Ricord says "it resembles greatly the ptyalism of pregnant women. The saliva is not viscous, and it seems not only to emanate from the mouth, but to be regurgitated. The mucous membrane of the mouth may be a little œdematous and irritable, but no inflammation or ulceration is perceptible, as in mercurial salivation, nor is the breath tainted, neither are the salivary glands the seat of any swelling."

Iodide of potassium augments considerably the action of the kidneys. One patient of M. Ricord, at the Hôpital des Veneriens, at Paris, while under its influence, passed daily from forty to fifty litres (the litre equal to 5-6ths of the imperial quart) of urine, drinking, however, in an equivalent ratio. This enormous secretion always ceased whenever the employment of the iodide was suspended. In its character the fluid differed little from ordinary urine, except in containing evident traces of the salt.

The circulation appears less influenced by iodide of potassium than most of the other functions; the pulse is commonly neither accelerated by it nor retarded. The instances in which the former condition has been remarked have been, for the most part, those in which a slight gastro-intestinal inflammation has been incurred. The iodide appears to exert an influence, however, on the physical constitution of the blood. This, under its influence, becomes less plastic; and nasal, pulmonary, intestinal, &c., hæmorrhages are more liable to occur and persist in persons of a scorbutic, scrofulous. or chlorotic habit of body.

Some symptoms, calculated to alarm practitioners not accustomed to employ this remedy, or frequently to witness its effects, are vascular injection and tumefaction of the conjunctiva, with œdema and infiltration of the submucous cellular tissue adjacent. There is little, if any, tendency to suppuration in this inflammation, and it is a symptom which most usually appears only at the commencement of a course of the iodide, and rarely recurs. But it is important to avoid mistaking it for a venereal or gonorrhœal ophthalmia, to which it appears to bear some analogy.

A somewhat similar affection sometimes extends under its use to the nasal mucous membrane, producing coryza, though unattended with sneezing, and without any disposition to pass into a suppurative stage. "The catarrhal mucus, provoked by iodide of potassium, *does not ripen* into matter of a viscous quality; and, indeed, if previously to the administration of the remedy there had existed a purulent secretion in the nasal fossæ, this secretion, unless it had its origin in a carious state of the bones, would, under the influence of the iodide, probably diminish and disappear. In some few individuals the effects above stated obtain in the bronchi, instead of the pituitary membrane or conjunctiva. But here, also, symptoms of any grave affection are absent; there is little cough or symptomatic fever; and, though a good deal of expectoration may take place, it is never of a suppurative kind.

The impressions of the iodide on the cerebro-spinal nervous system are sometimes perceptible in cerebral excitement proceeding from congestion, with symptoms like those of ordinary intoxication. At other times the motor nerves are slightly affected, giving rise to *subsultus tendinum* in a slight degree; and it is said that amaurosis has on rare occasions supervened.

The genito-urinary organs are among those which are most susceptible of the influence of the iodide of potassium. M. Ricord says, "I have been able to trace the fresh access of blennorrhœas ineffectually got rid of previously, or the exasperation of those actually existing, so frequently to the use of this medicament, that the existence or recent presence of a blennorrhœa I regard as its temporary contra-indication. But, perhaps (on the principle '*similia similibus*,' probably) advantage may be derived from the iodide of potassium in some cases of blennorrhœa."

Most of the inconveniences named above, as attending the employment of the remedy, do so in only a small proportion of cases; and few, even though having reached their acme, last a week after the suspension of the remedy, or a diminution of its dose. In a majority of cases, "not to lose time by inefficient treatment," M. Ricord advises the employment from the first of pretty large doses, that is to say, of six or seven grains, three times a-day, which doses may be doubled in the course of a week, if required. In his experience it has seldom been necessary to administer the remedy to a greater extent than about two scruples daily, or to a less extent than one scruple in the same period.

M. Ricord remarks that in cases in which both iodide of potassium and mercury are indicated, he prefers giving them on alternate days, or at other separate times, say at intervals of two hours apart, in preference to administering them in conjunction. His reason seems to be that the stomach tolerates each much better separately.—*L'Experience.*

ACCIDENTS IN FŒTAL DEVELOPMENT.

THE French Academie de Medicine was lately called upon by one of its members to bear testimony to the existence of a vesicular body, having the exact appearance of an embryo, from a month to six weeks from conception, in the centre of a placenta taken from a woman who had just given birth to a living and perfectly-formed child. The umbilical cord was implanted at the circumference of the placenta, and some of the veins communicating with it ramified upon the surface of the vesicle in question. This vesicle is a transparent kidney-shaped body, about the size of a hen's egg, and apparently containing an embryo floating in a liquid. It had not been further examined when presented to the Academy, but as it was about to be opened, further particulars may be published respecting this singular deviation from the ordinary course of nature.—*Bulletin de l'Acad., &c.* Dec. 31.

The readers of THE LANCET will recollect in the number for Dec. 3, 1842, an account of a fœtus which at its birth was marked in an extraordinary manner; a circumstance supposed to be accounted for by a fright which the mother had received during pregnancy. A somewhat similar incident is reported in a recent Parisian journal. A woman, married to one of the workmen employed on the Paris and Versailles railroad, was in the second month of her pregnancy at the time of the fatal catastrophe which happened on that road in May last. Hearing in the evening succeeding the accident of the explosion and loss of life, she hastened to the railway terminus in search of her husband, but not finding him she readily supposed he was among the individuals killed, in which belief she was confirmed when she beheld the blackened remains of the sufferers. However, her husband ultimately proved to be uninjured. Seven months afterwards this woman was delivered of a child, the back, arms, and thighs of which were marked with large black patches resembling, in some respects, the appearance of a burnt mass. The narrator of this circumstance in the "Gazette des Hôpitaux," starts off to ask, "Is this merely a simple coincidence? Is it, on the other hand, a proof of the influence of the maternal emotions on the fœtus?" Doubtless a grand argumentative oration is beginning with this magniloquent flourish! Not at all; the incipient Aristotle, frighted "ev'n with the sound himself has made," back recoils to the concluding sentence, "We content ourselves with stating the fact!"

CONTENTS OF THE SPLENIC VEIN.

To the Editor of THE LANCET.

SIR,—Mr. Eagle states that he was the first to call attention to the fact that the splenic vein *does not contain venous blood*, and that he now equally objects that it *does not contain arterial blood*. In raising this objection it is plain that he has read my paper hypercritically, and not attentively; because, at page 555 of THE LANCET, in anticipation of this criticism, I specially revert to the nutrition of the texture itself of the spleen, by which some *slight* change is necessarily produced in the large quantity of arterial blood which traverses the organ; for this is proved by its lymphatics. Nevertheless, the blood of the splenic vein is chiefly and essentially arterial, and would be wholly so but for this small portion of the residual blood of the splenic nutrition, which passes along the splenic vein with the rest, but which is not sufficient to invest the latter with the characters of venous blood, as on the whole it contains much less carbonic acid, &c.

In preparing for the Royal Society the arrangement of the paragraphs of my original manuscript, I accidentally omitted one of them, which I afterwards thought to be of no great importance, from the nature of the other details. But Mr. Eagle calls for it, and here it is:—"I took, in several phials, separate specimens of arterial and venous blood from a dog, as well as some from its splenic vein. There was not one surgeon to whom these were shown who did not judge the latter to be arterial blood." If there was any difference (which I allow there was), I have fully accounted for it in my paper, but did not think it necessary to render the latter complex by the discussion of a subject which was directly implied by the general argument. Now it will be seen that Mr. Eagle's objection does not militate in any degree against what I have advanced, and that he might have "saved his ink-shed" by reading attentively. I remain, Sir, yours, most obediently,

ROBERT STEVENS.

Kennington, Jan. 21, 1843.

THE SPLEEN.—MR. STEVENS' THEORY.

To the Editor of THE LANCET.

SIR,—Mr. Stevens, in his "Demonstration and Argument," is right in remarking that no change is effected on the blood in its progress through the spleen, but in every other essential particular he is wrong. He is wrong in stating that "the spleen comprises a mere congeries of capillary vessels," for the spleen, we all know, is remarkable for its distensibility, which *capillaries* are not. He is wrong, too, in stating that its capsule

is "a prolongation from the aponeurotic tissue of the diaphragm," for if it were so it could not be *elastic*—aponeuroses, or tendinous expansions, being *inelastic*. He is equally wrong in supposing the placenta to be "a double and deciduous body of capillaries between the uterine arteries and the umbilical vein, and the umbilical arteries and the uterine veins;" and in believing that blood not only passes from the mother through the placenta to the fœtus, but also back from the fœtus, through the placenta, to the mother; for if blood did return from the fœtus to the mother, it is evident the terminal branches of the umbilical arteries in the placenta would not be continuous with the radicles of the umbilical vein. He is, moreover, wrong in the version which he has given of *my* theory as to the use of the spleen; for he appears to imagine that I believe the spleen, *per se*, propels the portal blood through the liver; whereas, I have stated more than once in your columns that I do not regard the spleen as an isolated organ, but as the roots of an important vessel, the *spleno-hepatic vein*, and that it is *that vein* which propels the blood through the liver. But that in which Mr. Stevens is most wrong is in supposing the large size of the splenic artery is for the purpose of supplying a large quantity of oxygenised blood to the portal blood, or that the spleen is for the purpose of diverting arterial blood into the portal vein; for if it were so, the size of the splenic artery and the spleen would bear pretty nearly the same proportion to the liver in all animals, and would not be more than twice as large relatively to the liver in man as they are in the quadruped. The reason the splenic artery is a large artery is because it is necessarily proportionate to the quantity of *venous radicles* of which the spleen essentially consists.

I see the last number of THE LANCET contains a note from Mr. Eagle, in which he observes, that the blood in the splenic vein is neither venous nor arterial. Mr. Eagle is perfectly right *there;* it is neither venous nor arterial. *The portal blood is a diluted blood, for every ounce of fluid received by the stomach passes into the splenic vein.* I am, Sir, your most obedient servant,

JOHN JACKSON.

6, Stonefield-street, Islington,
Jan. 22, 1843.

HOUSE-SURGEONS AT INQUESTS.

To the Editor of THE LANCET.

SIR,—I beg to hand you the following statement of facts for publication, hoping that you will assist in rectifying the evil that they indicate:—A patient died in this hospital from the effects of an accident received at a village at some miles distance from Bath, and, owing to a recent decision of Lord Denman, instead of the inquest being taken here, it was held at the place where the accident happened, and I, as the house-surgeon, was compelled to attend the inquest, the coroner, without consulting my convenience as to time, warning me, at my peril, to be present at a certain hour. I was obliged to hire a horse at my own expense, and wander through lanes and by-ways up to my saddle-girths in mud and filth, and in a soaking rain, and on arriving at the scene of action the coroner comforted me with the information that the law expressly refuses me a fee, or even my expenses on the occasion, because I was a hospital surgeon! If this is not remedied I suspect that house-surgeoncies will soon be rather at a discount.

As regards the form of summons served on me to attend the inquest, I have to state that the coroner's officer waited on me, and read to me the usual summons for a witness from a paper. I was aware that I was not compelled to comply with the demand without receiving a special warrant from the coroner, but as the jury were collected, and my refusal would only have caused unnecessary trouble, as I should ultimately be obliged to attend, I appeared at the inquest, and told the coroner that I did not feel inclined to give my evidence unless with the usual fee. He told me that he could not give me either that, or even my expenses, as there was a positive prohibition against it, according to the statute, and that if I persisted in refusing he would be obliged to summon me, and in case of continued refusal I should be subject to certain severe penalties. Leaving the affair in your hands I am, Sir, your very obedient servant,

ARTHUR SARJEANT.

United Hospital, Bath, Jan. 10, 1843.

*** The notice which Mr. Sarjeant received to attend the inquest was not sufficient, in point of law, to enable the coroner to inflict any punishment upon him for the non-appearance at the inquest. We are of opinion, notwithstanding the decision of Lord Denman, in the case of the Great Western Railway, that the coroner for the city of Bath may legally hold inquests on the bodies of *all* persons who die in the institution named from the effects of injuries which have been inflicted upon them beyond the limits of that coroner's jurisdiction, and that the inquests in such cases would not be set aside, excepting in instances where felonies had been committed, as in case of murder and manslaughter, and those in which deodands had been imposed. This subject is becoming one of extreme difficulty, from the folly which has been committed in the Court of Queen's Bench, and an alteration must inevitably be made in the statute. When the common law is not supported by the dictates of common sense, it is high time that the Legislature interfered.

THE LANCET.

London, Saturday, January 28, 1843.

THROUGHOUT the modern history of medicine two very opposite but equally erroneous tendencies have existed in the minds of men. One class of inquirers has regarded the phenomena of life as resulting, in all instances, from laws entirely different from those which influence inert matter. Another class has resorted no less exclusively to the various branches of physical science, for the solution of vital phenomena which physical science is utterly inadequate to explain. Now, a moderate degree of unbiassed attention to the subject will be sufficient to convince us that, although many of the phenomena of living bodies are so totally different from any which are presented in the inorganic kingdom, as to justify the presumption that they must be distinct and peculiar in their nature, other processes are, nevertheless, carried on within living bodies, which, though conducive to vital ends, are, in their immediate nature, simply and purely physical. For example, when an irritable fibre contracts, in obedience to a stimulus, there is no physical law to which we can refer the phenomenon, and we, therefore, call it *vital;* but when the image of an external object is depicted on the retina, the process by which that is effected is as strictly referrible to the laws of optics as is the formation of a similar image on the tablet of a *camera obscura;* yet this physical part of the process of vision is just as essential to the existence of the sense, as those unknown vital actions by which an impression derived from the image is carried to the brain, there to enter into an endless variety of intellectual and moral combinations. Again, if we take the instance of respiration, we shall find vital, chemical, and mechanical forces combined in a function that is indispensable to life: here the *vital* action of muscles expands the chest; a *chemical* change is effected by the air on the blood, and expiration is aided by the *elasticity* of the thoracic parietes.

It should be observed, also, that where physical powers are made subservient to vital ends, these powers are dependent in their turn on vital agencies for the construction and maintenance of the apparatus through which they are manifested. Thus the figure, transparency, and refracting power of the lenses of the eye depend on a certain collocation of particles, which particles are deposited and preserved of the proper size, shape, and consistence, by the vital action of the vessels of the part, and a slight deviation from the normal action of those vessels may render the lenses opake, and abolish the sense of vision.

It were needless to say more in proof of the intimate connection and co-operation of vital and physical powers in the animal frame. It is sufficiently evident that the physiologist or physician who regards exclusively either the vital or the physical actions of the living body, takes a very circumscribed view of life, and one from which, in the present state of medical science, no important improvement can be expected to originate.

Although, at first sight, the study of the physical phenomena of life may be considered as more immediately interesting to the physiologist than to the medical practitioner, a closer inspection will convey a different impression, and teach us that some of the most important improvements in practical medicine which have been made of late years consist in the direct application of chemical and physical principles. To what does modern medicine owe more than to auscultation and percussion? Yet these are merely practical applications of acoustics to the detection of disease.

Where shall we find a more interesting fact in pathology than that which couples granular disease of the kidney with albuminous urine? Yet this is a simple and direct application of chemistry to diagnosis. Again, the obvious and legitimate tendency of pathology in the present day is towards a partial

revival of the humoral doctrines, and if so, what may we not expect for medicine from the boundless resources of modern chemistry?

But it must not be supposed that a simple acquaintance with the principles of natural science, and the functions of the living body, will at once enable the medical inquirer to apply the laws of physics to the solution of vital phenomena. Although the laws of inert matter are immutable, the conditions under which they come into operation in the living body are so peculiar that it would require much time and labour to learn how to apply them correctly. There are various instances in which phenomena, occurring in the living organism, must in reality be referrible to the known principles of natural science, but in which, nevertheless, we are compelled to acknowledge our inability to solve the phenomena according to those principles. For example, the ear is unquestionably an acoustic instrument, and we are well acquainted with its anatomical structure; yet the mechanism of audition is in a great measure unknown. The sounds of the heart are also, doubtless, referrible to the common laws of acoustics; yet the cause of the first sound is still unexplained.

While the careful application of physical science to the phenomena of life could not fail to yield the most beneficial results to medicine, the physical sciences would derive reciprocal advantage from the extended and modified application of their laws in a new and immense field of inquiry; many physical facts which are now isolated would probably become involved in useful trains of reasoning, and many sterile tracts of science would be fertilised. Hydraulics and acoustics, especially, would receive much illustration from a philosophical application of their principles to the phenomena of life. The study of vital hydraulics would lead to a closer investigation than has yet been made of the motion of fluids in elastic tubes, and animal acoustics would throw light on the propagation of sound through different media, and particularly through solid substances.

The physical phenomena of life have already been commented on at some length by M. Magendie, but he has taken hold of the subject by the wrong end, and fallen into so many errors and rash generalisations, that the study may be considered as not yet begun. It is obvious that the first step in the inquiry should consist in a careful examination of the physical and chemical properties of the textures of which organic bodies are composed—the relation of those textures to all agents capable of effecting physical changes in them—the variations occasioned in the physical condition of the textures by age, sex, and extraneous circumstances—and the diversities of the same texture in life and in death.

An accurate and practical acquaintance with the use of the miscroscope would be essential to such investigations. How far the vital actions of parts are modified by their minute physical structure, and how far the capillaries are immediately concerned in the intimate actions of life, or whether they merely convey the materials for actions which are extra-vascular, are questions of great moment in the inquiry which we have suggested, as well as in general physiology. It is to the microscope that we must look for satisfactory answers to them, and the daily improvements which are taking place in the construction of that instrument encourage the hope that it will ere long throw splendid light on the science of life.

We must content ourselves with this general exposition of the physical study of life—a subject replete with interest and utility, the prosecution of which we recommend to those practitioners whose acquaintance with natural science renders them equal to the task.

As the session of Parliament approaches, it behoves all who study the interests of humanity, and the good of our profession, to inquire what is to be done during its progress for the advancement of medical science

in this country, and the cause of Medical Reform. Session after session have these subjects been urged upon the Legislature, and always has a deaf ear been turned to the applications. But the time has arrived when neither the Government nor the people under its sway can afford any longer to blink the question. They feel and know that fact. By the Home Secretary a measure has been provided, which, whether it pass into a law, or be stifled in the birth (as it deserves,—concocted as it has been by an interested clique), at least indicates that the necessity for a reform in medicine has, at length, made itself heard; and that the oligarchical bodies which have so long and unjustly ruled the ranks of the profession, feel how impossible it is any longer to stem the pressure from without.

But while we are fighting the battle of the profession out of doors, is nothing to be done at home? Let medical men look within, and ask themselves how far their own advance in real science warrants their demanding that high professional consideration for which they contend. It is true that a great public measure which shall tend to reconcile jarring interests, to set at rest professional jealousies, and to withdraw the minds of medical practitioners from petty polemics and the warfare of class against class,—a measure which, by the union of all the members of our profession in ONE BODY, should effect all this, and which, sooner or later, must become a law,—would greatly spur on those whose duty it eminently is to place themselves in the wake of science, to read attentively the book of Nature. But an earnest or promise merely of what shall be done in future in this great, noble, and elevating undertaking, gives in reality no present claim to public aid. The public will not —they have no right—to be satisfied with a mere assurance that when the usurpations of medical bodies are set at rest, the medical men of this empire will devote their energies more fully to the essential knowledge of diseases and their means of cure, and to the keeping themselves fully abreast of the advancements in medical art and science which their continental brethren (*en masse*, we mean) have attained. The public will, on the contrary, demand, and justly, that medical men should already be in the possession of attainments which entitle them to its hearty support, before it will come forward, heart and hand, to obtain for them those privileges which the profession demand.

"It is a great blessing to know our own ignorance." True wisdom consists in knowing and owning it; the pretence before the world to that of which we are ignorant, is the vice of charlatans and mountebanks. It is no reproach to us, as followers and investigators of a science abounding with ever new and varying phenomena, that the causes of many of these lie still hidden from us; when NEWTON himself, whose objects of research were far more subject to fixed rules, and, perhaps, to positive demonstration, than our own, declared that he had been only like a child who gathers shells on the sea-shore while all the treasures of the vast expanse of ocean lay unexplored before him. And, like NEWTON, how little do we know, compared with what we have to learn! Every day develops new views, teaching us that many of what we before thought irrefragable truths, deserve only to be classed with bygone theories. Yet, dazzled by the splendour of great names, we still adhere to these; for such is the force of habit, that when once the human mind gets, as it were, into a rut, a generation at least must pass away before it learns to escape from it. Worse than no guide at all, is a bad guide. For, on the hypotheses which have been suffered to usurp the place of truth, a system of empiricism, under the name of routine practice, has been reared, vacillating, uncertain, and often pilotless, in the treatment of diseases, but which it will be the duty of a more enlightened system, based on organic chemistry, to overturn.

Against quacks and quackery we have always raised our voice; and to empiricism in any form we shall be merciless, whether it drag out a loathsome existence in obscurity, or rear its head in high places. For

pettifogging pill-makers and pilfering physicians we have an equal scorn. But the empiricism which has long prevented medicine from holding a place in the exact sciences, is only a part of that empiricism which has abased medical practitioners below the rank that they ought to hold in the estimation of the public. There is no shame, as we have before said, in being ignorant of many truths, the knowledge of which would light up with brilliant effulgence our onward career of practice; no moral degradation in knowing and acknowledging that we know little. But there is both shame and moral degradation in the petty, tricky, mean acts by which a medical practice is too often acquired; in the vain, shallow artifices by which the same practice is maintained, and the venal, mercantile manner in which it is conducted. The world will see through and fathom these arts. The rapid strides of learning among the people within the last twenty, nay, the last ten years, have greatly narrowed the former void between the medical profession and the public generally, as respects an acquaintance with philosophy and the sciences. And the public, at the same time that it has become nearly as well acquainted as the former with the workings of the material universe, has, in a proportionate degree, become better able to appreciate the exact amount of consideration which is due to the professors of medicine on behalf of their exact amount of superior acquirements. Conjointly with this, their eyes are more open to the *humbug* by which dust has been so long thrown into them. Truly, a reform in medicine must begin from within as well as without.

Memoire sur l'Anatomie Pathologique des Rétricissements de l'Urêtre, pp. 74; and *Memoire sur l'Emploi des Caustiques dans quelques Maladies de l'Urêtre*. Par le Dr. CIVIALE. 8vo. Pp. 59. Paris: Renouard. 1842.

THESE two pamphlets are properly consecutive the one to the other. In the first, M. Civiale enters into a brief sketch of the various kinds of obstructions occurring in the urethra, as valvular folds, excrescences, adhesions and induration of the mucous membrane, &c.; after which he proceeds to treat of organic lesions to which these give rise in the urethra, prostate, bladder, kidneys, and other parts of the genito-urinary apparatus. The third section is devoted to the consideration of false passage and its consequences.

In the second memoir M. Civiale runs rapidly over the history of the employment of caustic remedies for stricture, blennorrhœa, neuralgia of the neck of the bladder, &c., from the times of Alphonse Ferri and Loyseau, to our own day, canvassing their merits and those of the different modes which have been adopted for their application in these diseases. In the course of this M. Civiale details his own method of cauterisation in cases of stricture, but he is decidedly unfavourable to the general use of caustics in urethral disease; and in his peroration he has strongly deprecated their extensive employment against stricture, &c., so long prevalent in this country, but now, happily, for the most part abandoned.

The second of the two brochures will be found useful by the practitioner. In the first we fail to discover much that is new. It is worthy of remark, however, that nearly all the examples of disease cited by the author in this treatise, are described from what he has witnessed in the museums of *the London hospitals*, where the collections are so rich in specimens of disease affecting the genito-urinary organs, that M. Civiale remarks that nearly every disease affecting these organs may be here traced, step by step. This circumstance, however, he assigns to causes that are not creditable to English surgery, such as the wrongly-curved catheters which we employ; the little attention that is bestowed on the operation of passing them into the bladder; and, in particular, the frequent entrustment of this delicate operation to incompetent assistants. Making due allowance for the prejudices of a foreigner, there is much truth in his remarks on this subject, especially as regards the last, and we ought to profit by the public exposure of them, operating, at least, through our sense of shame, if not that of humanity.

UNIVERSITY OF ATHENS.—This institution still boasts of only two faculties, those of philosophy and medicine, each consisting of eight professors. At the opening of the present session somewhat more than fifty students had entered to the medical classes. —*Gazette des Hopitaux*.

PROOF, IN LAW, OF THE LICENCE TO PRACTISE AS AN APOTHECARY.

On the 11th inst., at Barnstaple, a Court of Record was held before the Recorder, when an application was made to the court for a new trial in the case of Joce *v.* Shapland, both of Swymbridge, which was tried at Barnstaple three months since, when the court ordered a nonsuit, on the ground of insufficiency of evidence to support the validity of plaintiff's licence as an apothecary. The attorney for the plaintiff, Mr. Carter, junior, having obtained of the registrar of the court a rule *nisi* for a new trial, applied to his honour to make the rule *absolute*. Mr. C., in support of his application, recapitulated the evidence which he had adduced on the trial to prove the authenticity of the licence of his client. The three medical gentlemen on whose testimony he relied were Mr. Cutcliffe, Mr. Smith, and Mr. Torr, who had practised several years under licences which they believed to be precisely similar to the plaintiff's, which they examined and certified to be genuine. He submitted to the court that on that evidence the case ought to have been allowed to go to the jury. Seven of the signatures to Mr. Cutcliffe's, and four to Mr. Smith's licence, were identical with the names on the licence of plaintiff. In the analogous case of handwriting, a witness was eligible to prove handwriting even though he had never seen the individual write; his having had correspondence with the party, and proving that the party had acted on the correspondence, was held sufficient to enable him to certify to the identity of the handwriting. The court, however, in Mr. Joce's case, had directed a nonsuit on the ground of the seal.

The Court: The genuineness of the certificate was required to be made out, and the court again and again suggested to the barrister who represented the plaintiff on that occasion to carry his proof as far as he could, but he chose to rely on the seal, and had not alluded to the signatures at all.

Mr. Carter said, that certainly his impression was that the court resolved that *the seal* must be proved by an officer of the company issuing it. In the case of Walmesley *v.* Abbott, the genuineness of the seal was not required to be proved, but it was held sufficient to prove the handwriting of one of the parties signing the certificate.

The Court: In that case much more evidence was adduced than in this.

Mr. Riccard, for the defendant, opposed the motion, hoping the court would refuse it with costs. Mr. Carter had endeavoured to impress on the court, that the evidence on the trial was sufficient to send the case to the jury; but the question raised was not one of *fact* for a jury, but of *law* for the court. He (Mr. R.) had no doubt of the genuineness of the certificate; but they must be governed by the rules of evidence; and the question was, had the plaintiff adduced such evidence as could justify the court in sending the case for trial, without introducing a principle which was dangerous to public justice? The court had decided in the negative, and there was no pretence for reversing the judgment; either the handwriting of the signatures, or the seal, must be proved. The plaintiff attempted the latter, failed, and was nonsuited. The case of Walmesley *v.* Abbott, put Mr. Carter quite out of court; for there the genuineness of the instrument was proved by witnesses who had been examined at the hall at the same time with the party, and had witnessed and could prove the handwriting. The decision in Chadwick *v.* Bunning laid down that proof was necessary either of the seal or the handwriting, and although the 6 Geo. 4, on which that case was decided, had expired, yet the same principle was held in Shearwood *v.* Hay (5 Adolphus and Ellis, 383), and Moyses *v.* Thornton (8th Term Reports), Sharp *v.* Wagstaff, and Simpson *v.* Disnure (Law Journal of April last). He submitted that the application must be refused with costs.

His Honour: There is no ground for the motion. On the trial three gentlemen were produced to prove the certificate; but they were unable to speak to it, excepting that, from a casual view of the seal, they conceived it to be like their own. I have no doubt that twenty persons in court could have asserted their belief of the genuineness of the instrument: I could have said so much myself; but that is not enough. The 6th George 4th has expired, but it is not unimportant to look at the causes which led to its enactment. It recited that the proof was attended with great difficulty; and this difficulty amounted to an impossibility, if several cases happened to come on for trial in different parts of the country at the same time. The 6th Geo. 4th was passed to obviate it, and I should have thought (but for a contrary decision) that its intention was to make the production of the seal sufficient evidence of the authenticity of the instrument. That Act has expired; but if under Act of 1815 (55th George 3rd,) it was enough to call practitioners of the neighbourhood to give such evidence as was produced in the present case, where was the "great difficulty" of which the 6th George 4th spoke? Is it not obvious that other evidence was contemplated? Collins *v.* Carnegie decided what sort of evidence the law required, and by that decision he had felt himself bound. The motion must, therefore, be discharged with costs.

*** We understand that it was universally considered in court, at the trial, that the Re-

corder's request to the advocate of the plaintiff "to carry his proof as far as he could" (the point upon which he was foiled), had entire reference to *the seal*. The announcements now, therefore, respecting the signatures, took the parties by surprise. The following is a copy of counsel's opinion, taken by the plaintiff, averring that there is no necessity in law for proving the seal, of which the Recorder was conscious it would appear by the above report, somewhat too late, however. The plaintiff could have proved the authenticity of the certificate, by comparing it with others than at hand, as well as the handwriting of the examiners, but his counsel was prevented by the Recorder's own words: —"I am quite clear that the seal must be proved." We may add, that Mr. Peacock is a barrister of experience and high reputation in his profession.

Opinion of Counsel on the Case.

"I am clearly of opinion that the plaintiff is entitled to a new trial, for I am of opinion that there was sufficient evidence to have been left to the jury of the authenticity of the seal. I think it was sufficient to prove the seal by a person like the witness Mr. Smith, who swore that he believed the seal to be that of the Apothecaries' Company, from his knowledge of the general character of the company's seal, derived from his having seen other impressions of the seal, which had been acted upon, and that it was not necessary for him ever to have seen the die, or to be able to describe it minutely. The same rule applies to handwriting, where it is sufficient for a witness who has never seen a party write to swear to his belief of the genuineness of a signature from his knowledge of the general character and appearance of the party's handwriting, derived from having seen the handwriting of the same party, or other instances where it has been acted upon.—See Phillips on Evidence, last edition, page 690, et seq., and it would not be necessary for him to be able to describe the form of each particular letter to entitle his evidence to be left to the jury.

"2ndly. I am of opinion that the authenticity of the certificate might have been proved by other means than by proving the seal.—See Walmesley and Abbott, 3 B. and C., 218, the stat. 55 Geo. 3, does not require the certificate to be under seal.

"I am of opinion that the court has power to grant a new trial.—See observations of Lord Mansfield, Bright *v.* Egnon, 1 Burr, Rep. 393. This is also done in the Palace Court, where the same judge, sitting in banco, reverses his own decision at nisi prius, and frequently grants a new trial. I presume that the action was commenced in the Borough Court, and was not tried there upon a writ of trial.

"B. Peacock."

"Temple, Jan. 4, 1843."

BRITISH MEDICAL ASSOCIATION.

Exeter Hall, Jan. 17, 1843, George Pilcher, Esq., in the chair.—A letter was read from Mr. Wakley, M.P., recommending determined opposition on the part of the association to any ill-devised measure for the regulation of the medical profession which might be introduced into the Houses of Parliament in the ensuing session. On the motion of Mr. H. Smith, seconded by Mr. Hooper, it was resolved, "That a deputation do wait on Sir James Graham to ascertain the course which the Government would pursue this year on the subject of medical reform." On the motion of Mr. George Bottomley, seconded by Mr. A. B. Wall, it was resolved, "That a deputation wait on the poor-law commissioners to request them to enforce the universal adoption of their late recommendation, by payment to medical officers on the *per-case* system." The members of the deputations were then named.

The council having completed some routine business, then adjourned.

ROYAL MEDICO-BOTANICAL SOCIETY.

January 11, 1843.

Dr. Farre in the chair.

PROXIMATE PRINCIPLES OF OPIUM.

Mr. Rodgers, the professor of chemistry, delivered a lecture on the proximate principles of opium. After alluding to the exceeding interest of the drug, in a physiological and therapeutical point of view, and the important results that have taken place in vegetable chemistry, from the discovery of its ultimate principle, morphia, he mentioned the comparative analysis of Smyrna opium by Mulder, who, after examining five specimens, gives the following as the average results of his inquiries:—Narcotine, 7.713; morphia, 6.228; codeia, 0.767; narceia, 9.000; meconia, 0.790; meconic acid, 6.121; fat, 2.209; caoutchouc, 4.338; resin, 2.753; gummy extract, 25.370; gum, 1.706; mucus, 18.733; water, 12.108. Two other principles are described by chemists—the thebaia by Couerbe, and pseudomorphia by Pelletier.

Narcotine is readily obtained from opium by the action of boiling ether, and is procured in crystals, as the ethereal solution cools. Other methods are occasionally adopted, but the above is the readiest mode of preparing it.

Morphia can be obtained from opium by

many processes; one of the best is that proposed by Dr. Gregory and Dr. Robertson, a modification of which is adopted in the present Pharmacopœia. The tests for morphia in the pure state are very decisive; the following are the best:—Nitric acid, when dropped on morphia or its salts, produces a red colour, which soon becomes yellow; neutral perchloride of iron produces a blue colour; iodic acid, when added to morphia, is deprived of its oxygen; the free iodine can then be detected by starch. The salts of morphia are more soluble than the alkaloid.

Codeia was discovered by Robiquet, in 1832; and Pelletier states that he obtained six ounces from one hundred pounds of Turkey opium. It differs from morphia in being more soluble in water, and by not being reddened by nitric acid, nor rendered blue by perchloride of iron.

Narceia was discovered by Pelletier, in 1832. It is obtained from opium by a very complicated process. It is, when pure, in white silky crystals, very sparingly soluble in cold water, soluble in alcohol, but not in ether. The action of acids is peculiar: when strong they decompose it, but when diluted they gradually combine with it, producing at first a blue colour, which changes into purple, and then red, and finally disappears altogether.

Meconia was discovered by Dublanc and Couerbe; it exists in very small proportion, and its properties require examination. Thebaia was also discovered by Couerbe; it is described by him to possess very strong alkaline properties. Meconic acid is peculiar to opium, and forms salts with various bases. It undergoes peculiar changes by the action of heat, and produces two new acids, viz., the metameconic and pyromeconic. On the addition of the per-salts it becomes of a red colour.

Much has been said about the tests for opium. It is one of the few organic poisons which admit of decisive detection; for if we can obtain proof of the presence of morphia by the tests already enumerated, and the red colour by the action of the per-salts of iron on the meconic acid, from a suspected solution, by the means recommended in various toxicological works, there cannot be a doubt of the existence of opium.

An animated discussion took place afterwards between Dr. Farre, Dr. Sigmond, and Dr. Cooke, on the relative medicinal value of the vegetable alkaloids, and the plants from which they are obtained.

The Chairman announced that at the next meeting, to be held on the 25th inst., Dr. Sigmond would deliver a lecture on the effects of some medicinal plants and their preparations in the treatment of asthma, and more especially during the paroxysm.

Jan. 16.—Anniversary Meeting.—The following gentlemen were elected officers for the ensuing year:—President, Earl Stanhope; treasurer, Mr. H. Cope, jun.; secretaries, Mr. J. Foote and Joseph Houlton, M.D.; conservator, Dr. Lane; librarian, Mr. E. Saunders; professor of botany, office vacant; professor of chemistry, Mr. J. E. D. Rodgers; of materia medica, Dr. Sigmond; and of toxicology, Dr. Cooke.

CURE OF ECTROPION BY NITRATE OF SILVER.

M. Magne was consulted on the case of a child ten years of age, of a feeble constitution and lymphatic temperament, who had for four months previously suffered ectropion of the left lower eyelid. The palpebral conjunctiva was thickened, and covered in its whole extent with granular excrescences; the eyelid was turned outwards as far as the Meibomian glands, and its secretion, which was more than naturally abundant, had caused an erythema of the cheek over which it fell. The ocular conjunctiva was also inflamed and thickened; the cornea did not participate in the inflammation. The use of the scalpel being interdicted in this case by the parents of the child, Dr. Magne was compelled to depend for the local treatment solely on caustic. This, however, he applied with great decision and vigour to both the palpebral and ocular conjunctiva; and to combat the increased inflammation he apprehended from this treatment he ordered frequent foot-baths, and compresses moistened with fresh alder flower-water, to be applied continually to the eye. The process of cauterisation was renewed daily for a fortnight, at the end of which period the granulation had disappeared, leaving only a few whitish eschars on the conjunctiva, and the child was soon afterwards sent away for change of air. Three months afterwards M. Magne again saw his patient, and reports that no trace of previous ectropion remained. On drawing forwards the lower eyelid two new membranous adhesions (brides), similar in appearance to the healthy membrane, were seen to have been formed near the external angle of the eye, and united the palpebral and ocular conjunctivæ, maintaining the eyelid in its proper situation, while the motions of the eyeball were not in any way interfered with. M. Magne has since treated other patients successfully by similar means. He states that in such cases, during the employment of the caustic, he finds it good practice to employ cupping between the shoulders, frequent warm foot-baths, and the affusion of cold water to the face and forehead, of course to prevent the access of local inflammation as much as possible.—*Gazette des Hopitaux.*

PROBABLE SUPERFŒTATION IN BILOCULAR UTERUS.

The "Svenska Lakare," a Swedish medical journal, publishes the case of a woman, forty years of age, who had had several living children, though her delivery had been always assisted by means of instruments. In the sixth month of a fresh pregnancy, she experienced severe hæmorrhage, which, though checked for a few days, returned accompanied with the expulsion of a dead fœtus, apparently of the third month. On examination, another fœtus was discovered, for the extraction of which the forceps became necessary. This fœtus was well developed, and appeared to be of the eighth month from conception; it was born alive, but died soon afterwards. The placentas in both births were sufficiently adherent to the uterus to require manual interference for their removal, in the course of which operation it was readily discovered that the uterus, almost from its orifice, was separated into two distinct cavities, in each of which a fœtus had been gestated.

POISONING BY VERDIGRIS.

A case of this kind is related by M. Dégrange, of Bordeaux, presenting marked peculiarities. A workman, with apparently no particular cause for suicide, complained on the 8th of August last, of much suffering, though without particularising its seat; and in the evening of the same day he was affected with vomiting and loss of appetite. Towards eleven o'clock he was found in his own house lying insensible on the ground, and removed to an hospital. Here he remained in a comatose state,—his face pale; respiration hurried; extremities covered with a cold sweat; deglutition difficult; abdomen not painful on pressure, and a total absence of either vomitings or purgings, till his death at four o'clock. The body after death presented externally little remarkable, except obvious and extensive congestion over all the head. It had been ascertained that he must have taken verdigris; and several masses of this substance, in a roughly-pounded form, were met with in the œsophagus, the mucous membrane of which presented inflammatory arborisations, with softening at intervals. In the stomach, where the poisonous substance was again met with, the mucous coat assumed a general green colour, and arborised engorgements of the small arteries were frequent in the great curvature, the mucous membrane of which was ulcerated in seven or eight places. Arterial arborisations, softening, ecchymoses, and a green tint similar to that in the stomach, extended through the intestinal canal in its whole length, and large collections of fæces filled the colon and rectum. Chemical analysis determined the presence of verdigris in these fæces (and it is said of copper in the contents of the urinary bladder), but none was discovered in the blood. The paucity of the vomitings and total absence of the purgings which usually supervene so violently after taking this poison, were the most remarkable features in the case detailed, and are supposed by M. Dégrange to have been due to the great abundance of the poison swallowed. We know that the effects of turpentine, calomel, and several other medicines, when they are taken to a large extent, are widely different from those which follow their administration in small quantities; and it would appear probable in the above instancethat instead of the violent efforts occurring that are usually made by the stomach to eject verdigris when taken in small quantity, the poison was suffered silently to erode the mucous membrane, and gradually to produce the apoplectic phenomena which destroyed life.—*Gaz. Med.*, Nov. 26.

NEWLY-REMARKED SIGN OF BONY CONSOLIDATION AFTER FRACTURE.

Dr. Guenther, of Hayna, in Saxony, relates the following case:—A young man fractured his right leg (both tibia and fibula?). Being of an observant turn, he soon noticed that while the nails on the toes of the left foot grew as usual, those on the right foot continued to maintain the same length, a fact which he had full means of verifying, as he had cut the nails on both feet immediately before his accident. Accurate observations on the state of the nails now continued to be made daily; and on the fiftieth day from the time of the fracture the nail on the little toe of the right foot began to increase in length; at which period, also, other ordinary signs of union in the broken bones first became evident. The nails on the three other small toes began successively to resume their growth, and lastly the nail of the great toe. Since the occurrence of this case, M. Guenther has witnessed similar phenomena in others, and he has laid it down as a rule in practice that the elongation of the nails is a definitive sign of union between the ends of bones of the extremities previously fractured.—*Medicinische Zeitung*, 1842.

THE SALTS OF QUINIA.

Prince Lucien Bonaparte has been making further researches on these medicinal agents. We have already once alluded to his experiments. He now recommends the employment in practice of both the lactate and valerianate of quinia in preference to the sulphate, the latter not producing those functional derangements in the nervous system which the sulphate sometimes causes; and the former on account both of its greater solubility and more energetic action. The fact, established by various physicians in

the Roman Maremme, that quinia alone, or its hydrate, is more efficacious as a remedy for intermittents than the sulphate, the prince considers due to its being converted into a lactate by the lactic acid of the gastric juice. This opportunity may be taken to mention (see Gaz. des Hôpitaux) that attempts have been made to combine quinia with ferrocyanic acid, and a substance entitled *hydro-ferrocyanate of quinine* has crept into pretty extensive use among French practitioners. But M. Pelouze has ascertained that this substance is in reality nothing more than quinine mechanically mixed with a little Prussian blue, the consequence of spontaneous decomposition of the acid.

NEWS AND CORRESPONDENCE.

We have received a letter signed "D. Cronin, Craven-street, Strand," in which the writer, referring to a notice to a correspondent in The Lancet of the 7th instant, intimates that the correspondence between himself and Dr. Venables, which had recently been advertised in the newspapers, relating to Dr. Cronin's treatment of phthisis by means of something called "traction," was a *bonâ fide* correspondence, and not got up in the usual way of quacks; at the same time referring to the professional standing and reputation, and the medical zeal and scientific productions of Dr. Venables, as proofs that Dr. V. was not likely to make one of a party in any unworthy scheme relating to the advertisement in question. Dr. Cronin intimates, in conclusion, that so far from the advertisement being "as little lucrative to him as honourable to Dr. Venables," he is really doing what we believe plain tradesmen would call "a great stroke of business;" and he invites us to "witness one of his morning levees," and inspect his "fee-book" in evidence of the fact. We observe that the last advertisement of this worthy gentleman is composed of a correspondence with a phthsical waiter at the *Cock* tavern in Fleet-street. Dr. Venables has got into good company. We must adhere to the opinion expressed on the 7th instant.

Mr. R. Stanton Wise, M.R.C.S.L., of Banbury, directs our attention to a report in a provincial newspaper of an inquest held upon a pauper, whose treatment fell into the hands of an uneducated person yclept a "bone-setter," and in whose charge the patient died. Mr. Wise asks "whether this is not a suitable case for the London College of Surgeons to take cognizance of," and, if they do not notice it, "of what use," he asks, "is their diploma, and what protection do they render to members of their institution?"—(Mr. B. Cooper and Dr. Webster gave replies for us to such questions, pages 589-95.)—"In this case the simple fracture of the thigh," says Mr. Wise, "was converted into a compound fracture, either by improper or insufficient treatment."

Extra-Licentiates.—Sir: Under "News and Correspondence," in the last Lancet, there was a communication whose object was to sneer at the extra-licentiates of the College of Physicians, while it proposed the question whether or not they were entitled to designate themselves "members" of the College? You very properly answer the question by saying that legally they are *not* so. But you know as well as I do that the intra-licentiates are no more "members" of the College than the *extra* ones, but as having some connection with the College, both classes have hitherto assumed the title of "members." I am not aware that any evil has arisen from that circumstance. I should like to know from your correspondent who "the old members in the provinces" are, who "scornfully repudiate" the extra-licentiates? Are they "fellows?" If so, their medical education is not certainly required to be *superior* to that required of the extra-licentiates. Or, are they "Scotch dubs," who have obtained the licence of the College to practise in England? If so, the qualification required of them and of the extra-licentiates is *precisely* the *same* under the late regulations of the College. Some few years ago the licentiates did not require so high a qualification as at present.

Jan. 23, 1843. Medicus.

An Old Subscriber.—The opinion of the witness was not worth a farthing. What effect, therefore, it should have had on the question under consideration need not be described.

A correspondent, referring to the assassination of Mr. Drummond, quotes from a Sunday paper the following passage:—Mr. Bransby Cooper, of New-street, Spring-gardens, said, "I went immediately, and found him on the bed and Mr. Jackson, *apothecary*, sitting by him;" and he requests to know whether Mr. Jackson is or not a member of the same College of Surgeons as Mr. Cooper, and if he be, how Mr. Cooper makes free "to designate as *apothecaries* gentlemen who not only possess the same *surgical* qualification as Mr. Cooper, but who have received a *medical* education in addition?"

The communication of *Dr. Page*, of Valparaiso, shall be inserted next week.

Pinx.—The fellow is smashed, and his carcase not worth an extra pinch.

B. G. sent his letter too late for the present number.

We are unavoidably compelled to postpone the promised republication of the *Medical Witnesses' Act*, and the letter of *Dr. Ayres*, until next week.

A Ten Years' Subscriber.—The works of Mr. Wardrop and Mr. Travers. On the second question opinions differ.